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COPPER SULFATE AS REAGENT IN CEREBROSPINAL FLUID ANALYSIS*

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A simple, dependable and time saving reaction for the tracing of deviations in the protein content of the cerebrospinal fluid has been the aim of experimentations for several years. A preliminary report in 1948[1] was based upon tests on various animals and cases of several clinico-pathological entities including neurosyphilis where this new reaction consistently gave accurate results. Since that time, more than 2,000 cases of routine spinal punctures and selected diagnostical groups were examined. The initial results have been confirmed and new facts were found.

Significance of Proteins in Cerebrospinal Fluid

In rendering a more detailed account of this work, it might be advisable to give a brief résumé on the biological and pathological significance of proteins in the cerebrospinal fluid. Various chemical compounds are contained in the cerebrospinal fluid, as water, inorganic salts, organic salts and their derivatives, carbohydrates, lipoids, both proteinoids and proteins, and cells. All these chemical compounds have their definite and specific task in the maintenance of the physiological equilibrium of the cerebrospinal fluid and they show reactive changes in case of disturbances caused by endogenic or exogenic noxious factors. Pathological reactions may arise in the cerebrospinal fluid that either evoke disarrangements in the proportion of some chemical constituents and in their interrelation, or are able to provoke compounds previously not present. Certain alterations in the central nervous system, of its parenchyma, interstitium or meningeal elements result in a consecutive increase, decrease or qualitative modification of different groups of the constant components of the cerebrospinal fluid. In cases of degenerative, involutive, nutritive or circulatory changes and of diseases caused by microorganisms, one or more of the chemical constituents suffer a reactive alteration which might be characteristic and even pathognomonic of the pathogenic agent.

Among the chemical compounds of the cerebrospinal fluid, the proteins may be altered and give the first useful sign in the great majority of cases. These proteins which assume the foremost biological and pathological importance, are traceable only in small amounts in physiological condition. The total protein percentage varies between 20 and 30 mg. per 100 cc. including all protein fractions.

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[1] "A new reaction in the biological and pathological analysis of the cerebrospinal fluid." Soutnwestern University Bulletin, 1948, vol. 48, 12: 1–9.

The proteins and the cerebrospinal fluid itself, as their carrier, have the physico-chemical properties of colloids. The proteins behave as the dispersed phase within the basic liquid of the cerebrospinal fluid which is their dispersion medium. In the size of the colloidal particles there are gradual differences below and over 100 millimicrons. The colligative properties of the cerebrospinal fluid, as the freezing temperature, the osmotic pressure and upon the chemical composition of the whole system. The freezing point is the viscosity, depend upon the physical quality of the dispersed phase and about —0.56° C. corresponding to that of a ⅓ molar solution with an osmotic pressure of cca. 8 atm. The osmotic pressure of the cerebrospinal fluid is practically determined by the NaCl proportion and only an irrelevant part of it is given by other electrolytes. The osmotic effect of the proteins may be disregarded at this point as their removal does not result in a considerable change of the balance. The viscosity, in turn, depends essentially on the concentration of the proteins. The correlation is similar to that of the variables of a quadratic (parabolic) function, if the dispersity and the proportion of the protein components are constant. Kjeldahl's method gives an average of 1.04 normal value in the cerebrospinal fluid while that of the blood is about 1.80.

The cerebrospinal fluid, being a colloidal system, has an appropriate static electric charge depending upon two factors: (1) the chemical constitution of the colloid and (2) the presence of electrolytes. The homonymic electric charges, also within the cerebrospinal fluid repulse, and the heteronymic electric charges attract one another. It is possible to change the electric charge of the colloidal particles and, in consequence, to change the fundamental properties of the cerebrospinal fluid as a colloid. The greatest possible change is the neutralization of the dispersed phase. This occurs in case of electronegative particles by the addition of cations and in case of electropositive particles by the addition of anions. An adequate portion of the ions will decrease the original electric charge to the limit of the isoelectric point where flocculation of the dispersed phase results. While approaching to the isoelectric point, the originally balanced counteraction of the homonymic, each other repulsing, charge of the colloidal particles is more and more hindered and finally paralyzed. The static electric charge of colloids containing proteins is like that of amphoteric solutions. In the presence of acid electrolytes the protein molecules join the H-ions and in the case of basic electrolytes the OH-ions.

In other respects the cerebrospinal fluid is like a hydrated emulsoid and it is dehydratable by means of adequately concentrated solutions of salts when the dispersed particles are deprived of their bound water and the previously separated particles adhere and gradually flocculate. The hydration of colloids is the resultant of several components as (1) the chemical constitution of the colloid, (2) the dimension of the dispersed particles, (3) the static electric charge and (4) the presence of additional electrolytes. The grade of hydration is of exceeding importance for the colloidal state. The less hydrated, *i.e.*, the more hydrophobic the protein particles are, the more instable is the colloidal system and flocculation results more easily. The globulin of the cerebrospinal fluid is highly hydrophobic.

The question of floccculation becomes more complex in case of the real suspensoids where protective colloids prevent the precipitation. Their presence increases the interfacial tension on the surface of the colloidal particles. Neutralization causes coalescence, however, an adequate additional electric charge hinders this change.

Radical changes as flocculation, precipitation and coagulation, beyond the borderlines of the typical colloidal solutions, in different ways, depends upon the mechanical, chemical or electric change applied, and, on the affinity of the dispersed phase to the dispersion medium.

Regarding the reactive chemical compounds, Hofmeister's lyotropic series supplies valuable information on the action of anions and cations upon the protein colloids. These chemical elements and compounds change the electric charge of the colloidal system, although the greater deal of their action aims to decrease its stability by depriving it of its solvent. Furthermore, after the dehydration of the dispersed phase, the members of the Hofmeister's series evoke the coalescence or the coagulation of the colloid.

A classification of colloids is possible on the basis of their stability, resp. their quality of resisting the precipitative action of physico-chemical interferences. Two groups have been established: the members of the (1) lyophobe series are less stable and tend to precipitate immediately when the static electric charge is changed; the (2) lyophil colloids in turn, resist any kind of exogenous change because of the greater physico-chemical affinity of the dispersed phase to their protective layer built up of the dispersion medium itself. If the criteria are adequately considered, as a consequence of the mutual interaction of the constant characteristics of both dispersed systems and chemicals, the grade of dispersity will decrease and finally the previously separated colloidal particles will become visible in the form of aggregates. But even in the case of advanced precipitation or sedimentation, the decomposition of the colloidal system, *i.e.*, the separation of the dispersed phase from the dispersion medium is not complete and only the protein particles tend to condense in larger and larger aggregates while the colloidal system as such remains.

At this point, the salts of heavier metals may be mentioned: they result an irreversible reaction with the physiologically relevant proteins and bring about precipitates insoluble not only in serum or in the cerebrospinal fluid but even in distilled water. Their application will be outlined later.

The physiologically important proteins present in the blood and in the cerebrospinal fluid include a large variety of different types and groups. The proteins themselves are built up of rather large molecules. Their molecular weight varies from 30,000 to 150,000 and their average diameter is about 4 millimicrons. The cerebrospinal fluid contains about 0.35 per cent of the plasma protein which is about 25 mg. per 100 cc. out of the 7,000 mg. per 100 cc. The osmotic equilibrium between the blood plasma and the cerebrospinal fluid is maintained by the high Cl percentage of the latter, cca. 720 mg. per 100 cc.

It is supposed that the liquor-proteins have their source in the same structures from where the cerebrospinal fluid originates: (1) the cerebral and spinal arachnoidea, (2) the chorioid plexus, (3) the ventricular ependyma

and (4) the parenchyma of the central nervous system. These proteins, like those of the blood, have diverse physiological functions and they are the specific carriers of chemical compounds infiltrating into the cerebrospinal fluid. These proteins act by their characteristic affinity to molecules of exogenous origin or to molecules derived of complex compounds present in the cells and in the tissues under physiological conditions. These colloidal proteins have the ability to establish a loose connection with non-physiological chemical compounds also. They do not separate before reaching the organs excreting or secreting them out of the organism together with the waste products of the metabolism. The pathological chemical compounds, even in the cerebrospinal fluid, arise of interferences between noxious agents and defensive reactions.

In analyzing the colloidal proteins participating in the defensive reactions of the cerebrospinal fluid we have to remember that these chemical compounds do not represent a uniform order of proteins but they appear in groups of molecules of various dimension and structure. Therefore, their classification is rather difficult. A classification on the basis of the number and structural order of the amino acids within the protein molecules is of little practical value, nevertheless, more useful is their physico-chemical classification.

The characteristic qualities of the proteins of the cerebrospinal fluid might be examined if the various groups are separated. The first reagent to be applied is usually the ammonium sulfate. According to the increasing concentration of the ammonium sulfate solution added to the cerebrospinal fluid, at first precipitate the larger molecules, being the least stable colloidal particles.

The cerebrospinal fluid does not contain fibrinogen, therefore, the first fraction among its proteins is the globulin group precipitating when the concentration of the reagent is raised to 28–32 per cent. The first member of the globulin group is the euglobulin, present in very small amount, if any. It is insoluble in distilled water.

The next fraction, the pseudoglobulin, precipitates at a concentration of 32–50 per cent of the ammonium sulfate and is regarded as a water soluble protein. The test of Nonne-Apelt-Schumm is based on its precipitation at 50 per cent. The pseudoglobulin molecules are much smaller than those of any one of the previous fractions. For their precipitation, it is not sufficient to eliminate the electrolytes but they must be deprived of their bound water attached to them by the oncotic or colloid-osmotic pressure which is about 25 Hg. mm. for a 1 per cent solution in water. The pseudoglobulins are composed mainly of glycine, tryptophane and choline, and contain a minor proportion of lysine and cystine. As far as they are present, as gamma globulin, their physiological task in the cerebrospinal fluid consists of the binding and transportation of immune bodies, and the complement function in a specific group of diseases.

The concentration of the pseudoglobulins in the cerebrospinal fluid increases in case of irritation, in several intoxications, inflammations, neoplasms, bacterial or parasitic infections and in a large scale of endogenous

and exogenous alterations. The concentration of the globulins varies from 2.5 to 9 mg. per 100 cc., or 10 to 30 per cent of the total protein percentage. The actual concentration in the human cerebrospinal fluid depends upon the anatomical region of the central nervous system. The intraventricular space contains 1–4 mg. per 100 cc., the cisternal level 1–6 mg. per 100 cc., and the lumbar sac contains about 2–8 mg. of globulin per 100 cc. of the fluid.

The last fraction precipitating at the complete saturation of the cerebrospinal fluid with ammonium sulfate is the albumin group. These proteins have but a less accentuated physiological importance. Their concentration amounts to 15–25 mg. per 100 cc. One usually determines their proportional correlation to the globulins and gives the result as the protein quotient 0.2–0.45 in average.

Obviously the globulin fraction has the most evident significance in the defensive mechanisms of the cerebrospinal fluid, and its reactive changes under pathological conditions are the most useful factors in tracing the interrelation between noxious agents and the organism, particularly the central nervous system. Alterations of other organs may interfere with the reaction of the tissues producing the cerebrospinal fluid, *e.g.*, the hypersecretion of the neurohypophysis increases, that of the thyroid gland inhibits its secretion.

The routine laboratory examination of the cerebrospinal fluid by means of the various protein reactions gives an orientation as to the changes of the globulin proportion. *Pándy's* test, with carbolic acid, is highly sensitive to globulins and might be positive also in case of extremely increased albumins. *Nonne-Apelt-Schumm's* test, using a 50 per cent solution of ammonium sulfate, offers a useful orientation as to the total protein percentage, however, without nuances. *Weichbrodt's* test, using a 1 per cent solution of mercury chloride, causes a flocculation of the globulins but the incidental presence of albumins influences the reaction. *Pawlowitsch's* reaction, using acetic acid and potassium ferrocyanate, is employed mainly in cases of specific meningitis. The test of *Donovan-Foley-Moloney*, using a 0.144 per cent solution of zinc sulfate, buffered with barbital, precipitates the globulin fraction, and, is accurate in cases of neurosyphilis and multiple sclerosis particularly.

Experimental. The here presented work aimed to bring about a colloid-chemical reaction concerning the increased globulin fraction of the cerebrospinal fluid under various pathological conditions. The investigations included both experiments with animals and cases of human diseases concerned with the central nervous system either in a direct or in an indirect way.

On consideration of the chemical facts outlined above—as a theoretical support to the experimentation—the work was started with examining various compounds of metals having an atomic weight of 55 to 207, except Hg. To a certain extent, higher chemical valences are more active than lower ones but the mem-

bers of the periodic system of chemical elements located on the extreme left are likely to have a more distinctive action upon the globulins. The most effective element was the copper (atomic weight 63.54) in the compound of copper sulfate. This is the "Cupric Sulfate Reagent Merck," $CuSo_4 . 5H_2O$.

The reaction with this compound is, like those mentioned above, fundamentally of a qualitative character. However, it offers an estimate for relative quantitative results by the chemical composition of the reagent and by the technique applied. The reagent is a 0.05 per cent (0.5 pro mille.) solution of copper sulfate in triple distilled water, with pH 5.

Technique. Pipette 0.5 ml. of the reagent in each of three small, serological, test tubes. This can be done in advance for each of the spinal fluids expected for examination. Add 0.25 ml. of the cerebrospinal fluid, drop by drop, into the first test tube, and, 0.5 ml. into the second test tube. When the cerebrospinal fluid is free of pathological changes, the contents of the first test tube will remain clear. The contents of the second test tube may turn opalescent when adding the last drops of the cerebrospinal fluid but it clears up spontaneously in a few seconds or after shaking. Add 0.75 ml. of the cerebrospinal fluid to the third test tube. The contents of this usually becomes permanently opalescent. In pathological cases the opalescence appears in the second and even in the first test tube. The result of the test shows up in a few seconds, however, after four to five hours, a sedimentation of the precipitate will develop in measurable amounts.

The substratum of the reaction, according to fractional precipitation and electrophoretic determination, is composed mainly of gamma globulin.

In a short time, one might gain sufficient experience in evaluating the reaction and will be able to draw an approximate quantitative inference. The practical usefulness of this test becomes evident when one compares the results of its repeated application during the observation of cases.

For comparison, one may mark the negative results with 0 (zero) and the more or less positive results, according to the appearance of the precipitation in the test tubes as follows:

negative:	opalescence in the third test tube
one plus:	opalescence in the second test tube
two plus:	opalescence in the first test tube
three plus:	opalescence in all test tubes and a cloudy-granular precipitate in one or more of those.

Animal Experiments. The copper sulfate reaction has been based on numerous experiments with dogs, rabbits, and guinea pigs:

(1) In four dogs 1.0 to 1.5 ml. of the cerebrospinal fluid was removed by sacral puncture and, after its temperature was kept at 60° C. for five minutes or at 10° C. for twenty minutes, reinjected. In both cases, two to nine days afterwards, a positive reaction of one to two plus was found, for a longer time in the cases where the higher temperature was applied.

(2) In three dogs and four rabbits 1.0 resp. 0.5 ml. of the spinal fluid was substituted by 25 per cent ethylalcohol. For from five to 23 days the reaction was positive, one to two plus. In two albino rabbits the positiveness was more accentuated than in grey ones.

(3) In four dogs 0.5 ml. of the cerebrospinal fluid was substituted by 25 per cent solution of NaCl in water. The copper sulfate reaction was positive from the second to the tenth day in two cases and to the 30th day in one case.

(4) Into four guinea pigs, one drop of Löffler's medium, two days old, was inoculated. The reaction was positive within eight to fourteen hours.

(5) In three dogs the insufflation of 1.5 ml. of air after the removal of 2.0 ml. of spinal fluid resulted in a slightly positive reaction within four to ten hours and endured for four to 18 days.

In several cases of groups (1), (4), and (5) the copper sulfate reaction was earlier positive than the test of Weichbrodt or Pándy.

Clinical Application. The copper sulfate reaction was tested in more than 2,300 cases of routine spinal fluid examinations and in various organic diseases of the central nervous system. In many cases, at different times, the cerebrospinal fluid was taken by both suboccipital and lumbar punctures. In some cases, additional determinations were made, as the quantitative analysis of the globulin fraction or dosing of the NPN or of the pH. For comparison of the results, at least one of the other globulin precipitation reactions was done in each case.

The following table shows the results obtained in 2,315 cases.

TABLE 1

Shows in a condensed form, the results of the diagnostical tests used in every pathological unit. The columns of the reaction of Pándy, Nonne-Apelt-Schumm, Weichbrodt, Pawlowitsch, Donovan-Foley-Moloney and of the $CuSO_4$ reaction indicate the percentage of cases which turned out positive of all cases where the test concerned was applied. The same is the case with the Wassermann reaction (and substitutes) and the colloidal gold reaction of Lange. The columns of the globulin, total protein, NPN, pH, NaCl, amino acids and cells indicate either the finding of one single case or the minimum and maximum values found in a larger number of cases.

Number	Diagnosis	Number of cases	Pandy	Nonne-Apelt-Schumm	Weichbrodt	Pawlowitsch	Donovan-Foley-Moloney	$CuSO_4$	Globulin mg./100 ml.	Total protein	Nonprotein nitrogen	pH	NaCl mg./100 ml.	Amino acids mg./100 ml.	Cells	Wassermann	Lange (Coll. gold)
1	Routine spinal punct.	1778	1.4	...	...	...	...	0.3	...	...	...	...	...	...	...	0	0
2	Meningismus	28	92	...	78	...	...	96	...	...	...	7.4	...	...	...	0	0
3	Encephalography	40	90	...	82	...	...	92	...	...	...	...	...	...	...	0	0
4	Subarachn. occlusion	1	100	100	100	...	...	100	39	...	...	...	...	...	...	0	0
5	Cerebr. arterioscl.	39	13	...	...	...	...	10	...	...	...	...	...	...	...	0	0
6	Meningitis csp. epid.	1	100	100	...	...	...	100	12	...	...	7.1	...	...	...	0	0
7	Meningitis tubercul.	5	100	100	...	100	...	100	... 6	... 24	14 21	7.2 7.5	...	...	...	0	0
8	Cerebral abscess	3	100	...	...	...	...	100	18	33	...	...	...	...	...	33	33
9	Poliomyelitis ant. ac.	29	90	75	...	...	...	83	...	...	...	...	...	...	...	0	0
10	Encephalitis letharg.	1	0	0	...	...	...	100	...	...	...	...	...	...	...	..	..

11	Postenc. Parkinson.....	22	9	9	...	...	...	9	...	...	...	...	...	...	...	0	0
12	Herpes zoster.....	5	60	40	...	...	...	80	6 10	...	...	...	...	...	...	0	0
13	Neurosyphilis, cer.....	343	96	83	74	...	94	97	9 184	...	13 18	...	...	...	2 1820	93	82
14	Neurosyphilis, tabes.....	24	79	73	73	...	100	83	6 14	...	...	...	...	...	5 430	78	83
15	Multiple sclerosis.....	10	80	...	70	...	90	80	3 14	...	...	...	...	...	...	0	0
16	Meningioma.....	3	100	...	33	...	...	66	...	...	...	...	...	...	...	0	0
17	Glioblastoma.....	1	0	...	0	...	...	0	4	...	...	...	...	...	...	0	0
18	Diabetic coma.....	3	66	...	...	...	...	66	...	...	...	...	...	...	...	33	33
19	Uremic coma.....	1	100	...	...	...	..	100	...	...	23 28	...	804 822	2.8 3.2	...	0	0
20	Encephalop. saturn.....	1	100	...	...	...	...	100	...	...	...	...	...	...	...	0	0

The cases of neurosyphilis represent the most important practical implications for globulin precipitation reactions. Table 2 brings the details concerning cases of general paralysis (dementia paralytica progressiva) and of tabes as to the percentage of the actually tested samples of cerebrospinal fluid.

TABLE 2

Gives a detailed comparison of the results of the various reactions in both types of neurosyphilis, groups 13 and 14 of table 1.

Neurosyphilis, G.P.I.	Pandy	Nonne-Apelt-Schumm	Weichbrodt	Donovan-Foley-Moloney	$CuSO_4$	Wassermann	Lange coll. gold
Number of cases tested	343	224	154	138	343	343	343
Number of cases positive	320	186	112	129	323	319	278
Positive cases in per cent	96	83	74	94	97	93	82
Neurosyphilis, Tabes	**Pandy**	**Nonne-Apelt-Schumm**	**Weichbrodt**	**Donovan-Foley-Moloney**	**$CuSO_4$**	**Wassermann**	**Lange coll. gold**
Number of cases tested	24	11	11	3	24	24	24
Number of cases positive	19	8	8	3	20	19	20
Positive cases in per cent	79	73	73	100	83	78	83

Two of the neurosyphilis cases produced an unusual combination of test results:

Case No. 21325, J. McC., 35-year-old colored woman. In 18 years, several series of antileutic treatment, including penicillin. Signs: Argyll Robertson pupils, dysarthria, dementia. With the exception of the exclusion test of Kline every reaction for syphilis, both in the blood and in the spinal fluid was negative, colloidal gold curve 1111100000. Copper sulfate reaction was two plus.

Case No. 28259, C. L. J., 22-year-old colored woman. In four years, several series of antileutic treatment. Signs: sluggish pupillary reaction to light and exaggerated tendon reflexes. Every test of the blood and spinal fluid negative for syphilis. Copper sulfate reaction was two plus.

In a considerable number of cases of neurosyphilis, followed up through years of observation and periodical antileutic treatment, an undulation was evident in the positiveness of the copper sulfate reaction. This shifting on the scale of pluses of the empirical graduation was in general accordance with the momentary condition of the patients, and, was often supported by a similar change of the specific syphilis reactions.

Summary

The purpose of this work has been to find an appropriate chemical compound for a sufficiently sensitive globulin reaction of the cerebrospinal fluid which reaction, to a certain extent, may be regarded as an approximate quantitative test indicating the grades of alteration during the course of observation. As a theoretical support of the experimentations, the colloid-chemical properties of the cerebrospinal fluid were outlined.

Experiments with animals were carried out and the reaction was used in comparison with other globulin reactions in more than 2,300 cases of routine cerebrospinal fluid examination and of 19 organic diseases affecting the central nervous system.

The reagent is a 0.05 per cent solution of chemically pure $CuSO_4 . 5H_2O$ in triple distilled water. 0.5 ml. of this reagent is pipetted into three small test tubes, and, 0.25 ml. of the spinal fluid is added to the first one, 0.5 ml. to the second and 0.75 ml. to the third one. The proportion of the spinal fluid to the reagent is 1:2, 1:1, and 3:2.

When no pathological alteration is present, an opalescence in the third test tube results. In cases of pathological changes with an increased globulin percentage opalescence or precipitation in heavier form results in the second and first test tubes, depending on the amount of the globulin, particularly gamma globulin, in the contents. In this manner, it is possible to draw certain conclusions as to the quantitative correlation of the case.

The application of this reaction succeeded in finding nuances in the globulin proportion, and, the reaction was positive in several cases where other globulin reactions did not give a definite result and where the anamnesis and clinical findings were concordant with its positiveness. This is a simple and time-saving reaction.

RENAL AND CARDIOVASCULAR EFFECTS FROM A SERIES OF N-SUBSTITUTED ARTERENOL DERIVATIVES COMPARED WITH ISOPROPYLARTERENOL (ISUPREL)*

CARROLL A. HANDLEY AND R. A. HUGGINS

Isopropylarterenol (Isuprel) has been reported to produce a fall in renal vascular resistance and an increase in renal plasma flow (Corcoran and Page, 1947). The depressor action and other systemic effects of isopropylarterenol have been extensively studied (Lands, *et al.*, 1947). A series of N-alkyl and N-aryl derivatives of arterenol has been demonstrated to dilate the bronchioles and produce vasodepression similar to isopropylarterenol, but with perhaps a more selective action on the bronchioles (Nuhfer, Seibert, and Handley, 1954). They have been compared with isopropylarterenol from the standpoint of the degree of vasodepression that could be tolerated without reducing renal function. Some cardiovascular effects have also been studied. These agents are: N-2-hexyl-(JB-226), N-2-(1-phenyl) propyl- (JB-230), N-2[1-(4-methoxy) phenyl] propyl- JB-245), N-(1-phenyl) propyl- (JB-246, N-[1-3,4-methylenedioxy) phenyl] propyl- (JB-251), N-1-(2-hydroxy)-propyl- (JB-253), and N-1-(2-hydroxy) ethyl- (JB-254) derivatives of arterenol.

Methods. Renal function studies were performed on 28 female dogs under pentobarbital anesthesia (30 mg./kg.). The effects of only one drug were observed during an experiment and several days elapsed before the same animal was used again. After 3 control periods of 10 minutes each, a 2 mg. per cent solution of one of the compounds stabilized with 0.05 per cent bisulfite was infused. The intravenous infusion was adjusted to produce graded drops in blood pressure. Total amounts of the various agents used during the period of observations are indicated in table 1. After a stable blood pressure was attained, observations on renal functions were made at two incremental depressions of blood pressure. Only observations made during the final, lower blood pressure level are recorded in table 1. In the final periods of drug

* From the Department of Pharmacology, Baylor University, College of Medicine, Houston, Texas. Supported in part by a grant from Lakeside Laboratories.

TABLE 1

Renal Response to Derivatives of Arterenol

Drug and Dog No.	Wt. kg.	Dose mg./kg.	MBP C mm. Hg.	MBP D mm. Hg.	UV C ml./min.	UV D ml./min.	GFR C ml./min.	GFR D ml./min.	RPF C ml./min.	RPF D ml./min.	FF C	FF D	RBF C ml./min.	RBF D ml./min.	RVR C	RVR D
JB-226, 1	14.4	0.2	150	88	1.4	0.2	29	4	75	..	0.39	...	115	..	1.30	...
" , 2	14.4	0.2	122	77	4.4	1.6	28	18	62	47	0.45	0.38	97	72	1.26	1.07
" , 3	13.0	0.05	148	114	1.7	1.4	40	38	99	131	0.40	0.29	171	226	0.87	0.51
JB-230, 1	15.4	0.6	115	71	2.6	3.9	41	39	94	113	0.44	0.35	136	161	0.85	0.44
" , 2	13.4	0.3	120	74	3.3	4.9	32	28	72	63	0.44	0.44	97	88	1.24	0.84
JB-245, 1	13.6	0.03	151	80	1.5	2.6	39	28	139	164	0.30	0.17	224	278	0.67	0.29
" , 2	15.6	0.03	159	92	2.0	4.0	45	42	120	144	0.38	0.29	203	244	0.78	0.38
" , 3	16.0	0.06	140	65	3.3	2.2	23	24	137	176	0.17	0.14	196	252	0.71	0.26
JB-246, 1	13.3	0.03	159	81	1.9	5.1	38	36	111	134	0.34	0.27	179	216	0.89	0.37
" , 2	10.4	0.03	169	72	3.2	3.2	43	29	121	117	0.35	0.26	208	177	0.81	0.41
" , 3	20.0	0.02	125	82	0.6	1.6	58	58	210	265	0.28	0.22	345	425	0.36	0.19
JB-251, 1	13.9	0.04	127	75	1.5	5.3	38	35	139	181	0.27	0.19	211	274	0.61	0.27
" , 2	13.8	0.04	133	89	1.9	4.7	55	54	152	193	0.36	0.28	234	284	0.57	0.31
" , 3	15.5	0.10	147	94	1.5	4.2	31	31	104	140	0.30	0.22	160	215	0.92	0.44
JB-253, 1	13.3	0.20	146	100	2.0	3.3	41	42	117	149	0.35	0.28	183	229	0.80	0.44
" , 2	13.4	0.60	140	82	2.6	4.3	42	38	111	153	0.38	0.25	188	243	0.74	0.34
JB-254, 1	21.0	0.20	98	63	3.3	5.7	37	32	125	130	0.30	0.25	169	167	0.58	0.38
" , 2	14.0	0.60	153	95	1.7	7.6	46	39	143	170	0.32	0.23	251	298	0.61	0.32
" , 3	15.7	0.20	164	103	1.5	6.6	34	52	138	206	0.25	0.25	238	361	0.69	0.29
Isuprel, 1	11.6	0.20	150	92	0.6	2.9	40	24	116	166	0.36	0.14	190	268	0.79	0.34
" , 2	9.4	0.20	160	93	1.1	0.6	38	11	...	...	...	...	...	...	...	...
" , 3	11.0	0.05	146	135	2.3	1.8	43	33	116	129	0.37	0.26	161	182	0.91	0.74
" , 4	10.0	0.10	144	127	4.1	2.9	33	22	95	102	0.35	0.22	132	144	1.09	0.88
" , 5	10.5	0.10	145	104	3.4	2.7	40	23	109	119	0.40	0.32	155	173	1.07	0.83

MBP—Mean blood pressure.
UV—Urine volume.
GFR—Glomerular filtration rate.
RPF—Renal plasma flow.
FF—Filtration fraction.
RBF—Renal blood flow $\left(\frac{\text{RPF}}{1\text{-Hct.}}\right)$.
RVR—Renal vascular resistance $\left(\frac{\text{MBP}}{\text{RBF}}\right)$.
C—Control (av. of three 10-min. periods).
D—20–40 min. after continuous i.v. infusion of drug (av. of two 10-min. periods).

observations, an attempt was made to adjust the rate of infusion so as to produce a 50 mm. Hg. fall in blood pressure from the control level. Creatinine was used to estimate glomerular filtration rate (GFR), and p-aminohippurate for renal plasma flow (RPF). Chemical methods for estimation of these substances and techniques have been described (Handley, *et al.*, 1949). A needle in the femoral artery was connected to a manifold for withdrawing arterial blood samples and for recording mean blood pressure with a mercury manometer.

Cardiovascular studies were done in another group of animals. Sounds connected to manometers were passed down the left carotid to obtain aortic pressure pulses and down the right jugular to measure either right auricular or ventricular pressures. Aortic pulse contours were used for determining systolic, diastolic and mean blood pressure, and for estimation of the cardiac output (Hamilton and Remington, 1947).

Results

Table 1 presents data taken from experiments on each of the compounds. Six renal function studies were done with each of the compounds, except JB-230, JB-253, and Isuprel. Only the two experiments, indicated in table 1, were done with the former compounds and ten observations on Isuprel were made. The data shows that the mean blood pressure may be reduced to approximately 70–80 mm. Hg. with most of the compounds without seriously impairing renal functions. In fact, there was a tendency for the RPF and urine output to increase, although the latter effect was probably due to fluid infusion. The calculated renal vascular resistance was reduced considerably in most experiments. The N-hexyl- (JB-226) derivative of arterenol and Isuprel appear to depress renal functions at higher mean pressures than the other compounds. If the mean blood pressure is reduced much below 100 mm. Hg. with these two agents, GFR and urine output are depressed. The reduction in renal vascular resistance was not as prominent with these compounds as with the others in the series.

Cardiovascular hemodynamics. The pulse contours produced by all these arterenol derivatives are similar to those produced by Isuprel (Fig. 1b). Qualitatively the effect of all the agents are similar. They reduce the mean blood pressure, but the diastolic pressure falls more than the systolic. Cardiac acceleration and an increase in output occur associated with a reduction in peripheral resistance (Table 2). As the dose is raised, there is

a rise in right auricular and ventricular pressures indicative of incipient cardiac decompensation. The electrocardiographic changes under these conditions suggest a relative myocardial ischemia (Fig. 2b).

TABLE 2

Effects of Some Derivatives of Arterenol on Blood Pressure, Heart Rate, Cardiac Output and Peripheral Resistance in a Series of Dogs

Drug	Dose μg./kg.	Systolic Diastolic	Cycle Time m. sec.	Cardiac Output ml./sec.	Peripheral Resistance[2]
Isuprel	C[1]	106/52	748	43	1.3
	1.0	50/24	238	77	0.2
JB-226	C	100/62	561	54	1.1
	1.0	92/46	238	64	0.7
JB-230	C	174/126	323	53	2.4
	1.0	156/96	204	59	1.7
JB-245	C	170/132	391	45	2.9
	1.0	122/70	285	72	1.0
JB-246	C	110/60	697	59	1.1
	1.0	88/26	323	76	0.4
JB-251	C	122/70	672	55	1.3
	1.0	104/44	357	70	0.7
JB-253	C	166/136	357	36	3.5
	1.0	134/90	221	39	2.3
JB-254	C	176/40	306	50	2.7
	1.0	142/94	204	70	1.4

[1] C = Control.
[2] Peripheral Resistance — $\frac{Rp = Pm\text{-}20}{C.O./sec.}$.

The vasodepressor action of Isuprel is greater than the other members of the series.

Discussion. The effects of these compounds on renal function suggest a certain amount of selectivity among them on different vascular beds. The observation that Isuprel and JB-226 reduce the GFR and increase the RBF little or not at all while the mean blood pressure is relatively high indicates that the kidney does not participate as fully in the vasodilation as with the other compounds. This interpretation is substantiated by the calculated renal vascular resistance (RVR) which shows a much smaller reduction after the administration of Isuprel and JB-226. On the contrary, the RVR is consistently reduced by 50 per cent or more by the compounds that have little or no effect on renal functions. These agents apparently produce sufficient renal vasodilation to

maintain, or even increase renal blood flow, in spite of a considerable reduction of the mean blood pressure.

The increased urine flow frequently encountered after the administration of most of these compounds is probably osmotic diuresis from the infusion fluid.

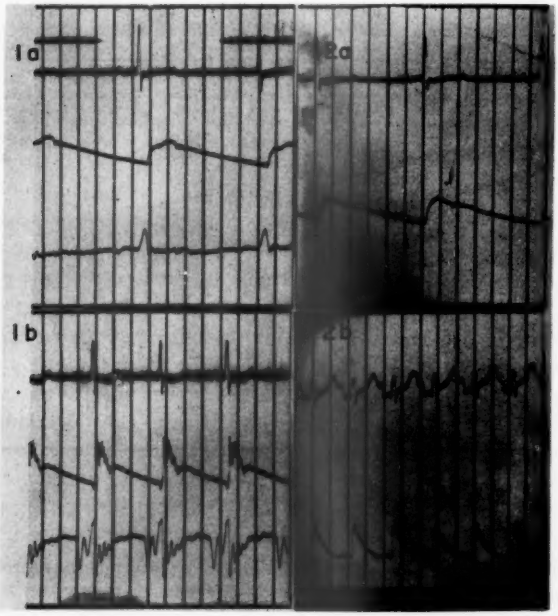

FIG. 1a. Control values for E.C.G. (Lead II), aortic and right auricular pressures. Vertical time lines equal to 0.1 sec.
FIG. 1b. After 1 μg./kg. of JB-226.
FIG. 2a. Control values for E.C.G. (Lead II), and aortic pressure.
FIG. 2b. After 5 μg./kg. of JB-245.

N-substituted arterenol derivatives

Electrocardiographic changes from the larger doses of all the compounds resemble those seen after partial myocardial ischemia. Although Isuprel is known to dilate the coronary arteries (Konzett), coronary vasodilation is apparently inadequate to compensate for the rapid heart rate, increased cardiac output and lowered systolic pressure seen after the administration of

Isuprel. Larger doses of the JB series are required to produce these effects (Table 2).

Summary

Replacement of the N-hydrogen of arterenol by 2-hexyl, 2-(1-phenyl) propyl, 2-[1-(4-methoxy) phenyl] propyl, 1(1-phenyl) propyl, 2-[1-(3,4-methylenedioxy) phenyl] propyl, 1-(2-hydroxy) propyl, and 1-(2-hydroxy) ethyl groups results in strongly depressor compounds similar to Isuprel. Small amounts of Isuprel and the N-2-hexyl derivative of arterenol tend to reduce the renal vascular resistance (RVR) and increase renal blood flow. Larger doses that reduce the mean blood pressure much below a 100 mm. Hg. have little effect on RVR and tend to produce partial renal ischemia, and a decrease in GFR and urine output. The pressure may be reduced to about 70–80 mm. Hg. by the other derivatives of arterenol with a sufficient reduction in RVR to maintain renal blood flow or to actually increase it. Glomerular filtration rate is usually not greatly changed. All compounds tend to increase the cardiac output and produce cardiac acceleration, but these effects are not as prominent as with Isuprel.

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A NOTE ON THE ARCHITECTURE OF THE MITOCHONDRIAL CORTEX AS SEEN IN TISSUE CULTURES OF NERVE FIBERS*

K. HAYDEN,† C. M. POMERAT, AND McDONALD SMITH

One of the simplest, most inexpensive and quickest methods of obtaining rapidly growing nerve fibers for analyses of axoplasmic activity consists of cultivating transverse sections of chick embryonic spinal cord *in vitro*. The technique together with a diagram of the essential steps in the procedure has been described by Pomerat (1951) and examples of the use of the preparation for determining the toxicity of drugs known to influence the activity of the nervous system has been reported by Painter, Pomerat, and Ezell (1949) and by Pomerat and Leake (1954).

A comprehensive study is in progress dealing with the biology of the spinal cord of chick embryos in tissue culture. Provisionally, it has been established that the 9th day of incubation yields particularly useful explants. At that time the cord has reached a size and tensile strength adequate to facilitate ease of handling, neuronal differentiation is adequate to produce a rich feltwork of outgrowing neurites, yet meningeal organization has not yet progressed sufficiently to embarrass the outgrowth with a large number of spindle cells. While clot liquefaction beyond the 3rd day of culture progressively disturbs the organization of the outgrowth, axons regularly have been found to emigrate as much as 1.6 mm. within the first 48 hours of incubation.

Neurites *in vitro* are characterized by angular bends along their course and by small aggregations of axoplasm which are often designated as *varicosities*. These are considered normal but they appear to be excellent indices of fiber injury. Following heat changes, variations in the composition of the nutrient medium or mechanical disturbance, enlargements of the varicosities develop which are described as *blisters*, blebs or local vacuoliza-

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tions of the axonic process. The susceptibility of neurons to injury in tissue cultures has been cited repeatedly by Levi and his pupils (*cf.*, Lumsden, 1951). Of special interest in this connection is the fact that the form and activity of mitochondria are also known to provide evidence of cell damage. In the segments of axons between blistered varicosities at the beginning of nerve degeneration, the presence of mitochondria of the type considered typical for healthy cells and showing characteristic motion in moving picture records leads to the interpretation that the aggregations of axoplasm (varicosities) represent the most vulnerable point of injury.

Figure I represents 8 frames selected from a motion picture record of two blisters along the course of a damaged nerve fiber in a 24-hour culture of 9-day chick embryonic spinal cord. On several occasions bodies having the shape of gelatin capsules as seen in figure I g (open arrow) were observed. In this cinematographic sequence it is believed that identification as a damaged filamentous mitochondrion could be assured. The entrance of a typical mitochondrion from an apparently undamaged segment of the axon can be traced in figure I, a to d (dark arrow). Successive film frame records (Fig. I, f to h) of the progressive swelling of this mitochondrion provided stages in the sequence of transformations unquestionably leading to the form change exemplified by the structure shown by the open arrow in figure 1 g. The blisters show similar stages in other mitochondria, especially in the bleb on the right side of figure 1 h. Whether the change is due to a difference in osmotic pressure within the zone of injury forms part of current experimental studies on the effects of electrolytes on the organoids of living cells.

The diameter of the uninjured mitochondrion as seen in figure 1 b was calculated to be approximately 0.8 of a micron. The axoplasm at that point was of only slightly greater diameter. This relationship, which is regularly encountered in our preparations, raises important questions concerning the movement of nutrients along the course of the axon. The diameter of the mitochondrion indicated by the open arrow in figure 1 g measured 2.9 microns. From this it was calculated that the cross-sectional area of the uninjured mitochondrion was approximately 0.5 microns^2 while that of the injured one was 6.6 microns^2, the increase being of the order of 13 times. Of course,

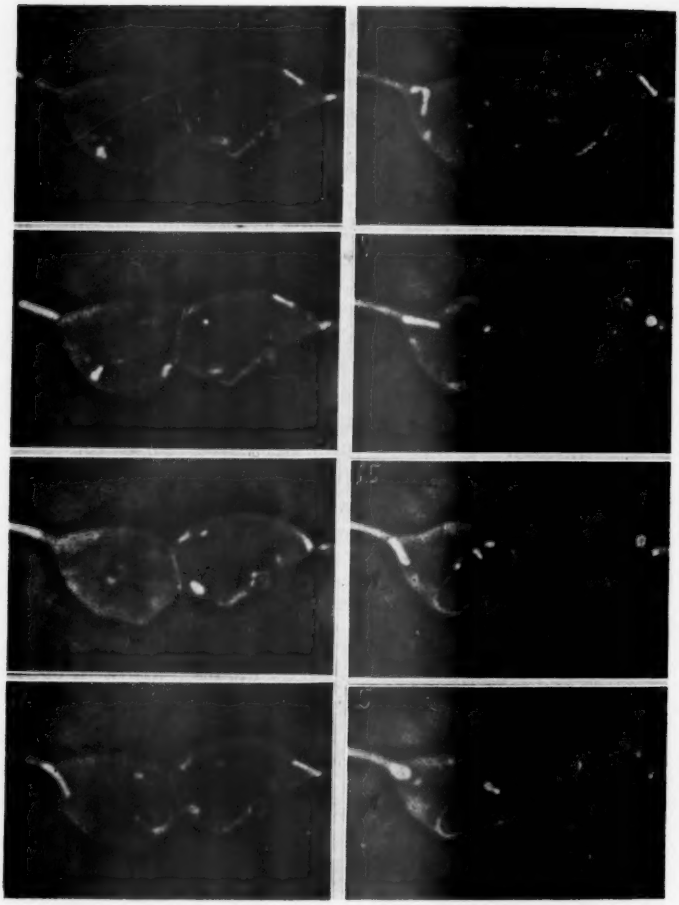

FIG. I. Selected frames from a phase contrast, time-lapse cinematographic record of a 48-hour neurite outgrowth from the spinal cord of a 9-day chick embryo. Two blisters probably having resulted from local injury to axoplasmic varicosities are shown with what are considered to be uninjured segments of the axon on either side. Progressive changes in the form of a mitochondrion as it enters the zone of injury is indicated by the solid arrows. It is believed that such transformations lead to the enormous distention of mitochondria, such as that indicated by the open arrow in *g*.

The scale of magnification for the series is given in the upper right-hand area of *a;* numbers in the upper left-half field from *b* to *h* indicate the lapse of time from the preceding photograph.

this assumes that the diameter of mitochondria in uninjured axons is relatively constant.

The morphology of the swollen mitochondrion is interesting in showing that its long axis is straight sided. This is believed to reflect the underlying molecular architecture of the mitochondrial cortex. Palade (1953) has reviewed recent data obtained on the structure of mitochondria as revealed in electron micrographs of tissues after treatment with a well buffered fixative. Evidence has accumulated to establish that internal shelves or crista extend into the lumen of the mitochondria from their cortical wall. Careful study has not yet revealed these in our photographs of living cells although the engaging possibility has been considered that the increase in the diameter of swollen mitochondria might result from the extension of crista if they consist of internal folds of the mitochondrial wall; that is, if the assumption be proved that the mitochondrial cortex is built like a camera bellows. Further studies with improvements in the optical setup and with preparations made under controlled osmotic conditions are in progress.

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HISTOLOGICAL AND ENDOCRINOLOGICAL OBSERVATIONS IN TISSUE CULTURES OF POSTERIOR PITUITARY OF DOG AND RAT*

WALTHER HILD†

In a series of papers (Hild and Zetler, 1951a, b; 1952a, b; 1953a, b) evidence has been accumulated in favor of the premise that the so-called posterior lobe hormones do not originate in the posterior pituitary itself, but are produced by the neurosecretory cells of the supraoptic and paraventricular nuclei of the hypothalamus. The hormones (vasopressor, antidiuretic, and oxytocic) are transported within the axons of the neurosecretory cells (tractus supraoptico-hypophyseus) to the neurohypophysis where they are stored and released when needed. Everywhere in the hypothalamic-hypophyseal system the hormones are associated with a stainable carrier substance which can easily be separated from the hormones by extraction of the fresh tissue with organic solvents in which the stainable component of the neurosecretory substance is soluble (Hild and Zetler, 1953a). The posterior lobe, therefore, is not an endocrine gland in a strict sense, but an organ of storage for the neurosecretory material formed in the hypothalamus.

This concept contrasts with the older theories concerning the site of origin of the vasopressor, antidiuretic and oxytocic hormones. Herring (1915), followed by Schönig (1926), Cushing (1933), Marcano (1935), Meesen (1935), Collin (1937), Racadot (1949), and Beck (1952), believed that the hormones in question are formed by basophilic cells which immigrate from the adenohypophysis into the neurohypophysis where they undergo degeneration; the hormones were thought to be produced as a

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result of this degenerative process. Other investigators (De Lawder, Tarr, and Geiling, 1934; Gersh, 1937, 1939; Fisher, 1937; Ranson, Fisher, and Ingram, 1938) were of the opinion that the hormones are elaborated by specific gland cells in the posterior lobe, *i.e.*, the "pituicytes" described by Bucy (1930). All these authors refer to the "pituicyte" as a special secretory cell of the posterior lobe and seem convinced that these cells are innervated by the nerve fibers of the supraoptico-hypophyseal tract. Many observations, as for instance those of Ranson and his coworkers (Fisher, 1937; Ranson, Fisher, and Ingram, 1938; Magoun, Fisher, and Ranson, 1939) have been interpreted on the basis of this hypothesis.

However, if the pituicyte were a glandular cell, it should give histological evidence of secretory activity as do the cells of other endocrine organs. Gersh (1937, 1939) and Gersh and Brooks (1941) report such findings. These authors describe lipoid droplets in the cytoplasm and processes of cells in the neurohypophysis and believe to have observed an increase in the number of these inclusions in dehydrated animals. Their findings were, however, not confirmed (Hickey, Hare, and Hare, 1941; De Robertis and Primavesi, 1942; Desclin, 1947). In a recent paper Leveque and Scharrer (1953) show that the cells of the posterior lobe do not produce the stainable neurosecretory substance which, according to Hild and Zetler (1953a, b) contains the posterior lobe hormones. There is, in fact, serious doubt whether the cells of the neurohypophysis are sufficiently different from glia cells in general to deserve a special term (Rasmussen, 1938; Vazquez-Lopez, 1942, 1953).

The only evidence so far unchallenged in favor of a production of posterior lobe hormones by cells of the neurohypophysis is based on the experiments of Geiling and Lewis (1935), who demonstrated the presence of vasopressor hormone in tissue cultures of neurohypophysis of mouse and rat, and the findings of Anderson and Haymaker (1935–36), who tested posterior lobe tissue of rats grown *in vitro* and found antidiuretic, but no oxytocic hormone. It seems reasonable to conclude, as did for instance Griffiths (1938), that if the cells of the posterior pituitary are capable of producing hormones *in vitro*, they can probably do so *in vivo* and should therefore be viewed as the sources of the posterior lobe hormones.

There would seem then to exist a disagreement between the observations which indicate that the posterior lobe hormones are not produced by cells of the neurohypophysis, and the findings of those investigators who studied these cells in tissue cultures. The discrepancy could be explained by the assumption, discussed in previous papers (Hild and Zetler, 1951a, b) that the hormones which had been shown to be present in the cultures of Geiling and Lewis by means of bioassays, had been carried into the culture medium together with the "storage tissue" and had not yet been inactivated at the time the bioassays were performed. The following experiments were undertaken, therefore, in order to determine (1) whether the posterior pituitary is really able to produce hormones *in vitro* as claimed by Geiling and Lewis (1935), Anderson and Haymaker (1935–36), and Griffiths (1938) and (2) whether it is justified to postulate on a histological basis the existence of a special type of a secretory or glia cell in the posterior lobe, *i.e.*, the "pituicyte."

Material and Methods

In preparing the tissues from rats and dogs the complete separation of the posterior lobe from the other parts of the hypophysis, especially from the closely investing intermediate lobe, was essential, because culture of the posterior lobe should not be contaminated with intermediate lobe cells. The following method for the separation of the two parts has proved useful: after enucleation of the posterior and adherent intermediate lobe from the groove of the anterior lobe in which they lie, the posterior lobe is rolled to and fro, for at least one minute, over the rough dry surface of a large frosted slide. As a result of this procedure the cells of the intermediate lobe come off or become at least so damaged that they can be rinsed off the posterior lobe by washing the latter with balanced salt solution.

The posterior lobe tissue, freed in this manner from adherent intermediate lobe cells, was cut in small pieces for cultures (roller tube technique). The solid medium by which the tissue is held to the cover slip, consisted of one part chicken plasma and one part embryonic extract; the fluid medium was composed of 45 per cent Gey's balanced salt solution, 50 per cent human ascitic fluid and 5 per cent embryonic extract. The fluid medium was changed every 4 days. The cultures of rat organs were grown for 4 and 7 days, those of dog organs for 4, 5, 8, 9, 10, 11, 13, 17, 21, 24, 27, and 32 days. The dog tissues were all derived from normal dogs; the rat material was taken (1) from normal rats, and (2) from rats the posterior lobe of which had been emptied of neurosecretory substance by intraperitoneal injection of a 5 per cent NaCl solution (5 cc. per 100 gm. body weight daily) for 4 days. The culture material was studied after fixation in Bouin's fluid and formol-bromide and staining with Gomori's chrome alum hematoxylin phloxin method,

Jacobson's stain and Bodian's silver impregnation. Moving picture records were taken with phase contrast for the study of the behavior of living posterior lobe cells in comparison with the behavior of glia cells derived from hypothalamus and cerebellum.

Bioassays for the detection of "posterior lobe hormones" in the cultured tissues and in the culture medium were performed as described by Hild and Zetler (1951b). Posterior lobes removed from normal and dehydrated animals without having been cultured *in vitro* served as controls.

Results

Sections of posterior pituitary from normal rats show the neurosecretory material derived from the nerve cells of the supraoptic and paraventricular nuclei as granules and droplets distributed all over the organ. The hormone content of the posterior lobe tissue can be estimated since it is directly related to the amount of neurosecretory material (Hild and Zetler, 1951b; 1952a, b; 1953a, b). The fate of this material in posterior lobe tissue cultivated *in vitro* for a shorter or longer period is, therefore, of interest.

The outgrowth from the original explant began about 24 hours after the roller tubes were placed in the incubator. First to appear were spindle shaped cells which migrated radially into the solid medium. After the 4th day of cultivation, the outgrowth covered an area about 4 to 6 times larger than the original explant. By this time, the latter had undergone a process of flattening caused by the death of some cells and the emigration of others and by the degeneration and gradual disappearance of nerve fibers, so that the culture could be examined histologically as a whole with little difficulty. In most cases, only a small area of the original explant was so thick that the light could not penetrate it. After 7 days the flattening process was even more advanced, and it was not necessary, therefore, to prepare sections for microscopic study since the whole culture could be studied. Four-day-old cultures stained with Gomori's chrome alum hematoxylin phloxin showed a remarkable loss of neurosecretory material, the remainder of which was clumped together in the area of the original explant; it was never found in the zone of outgrowth. Since in the intact posterior lobe the neurosecretory substance is bound to the nerve terminals, and since the nerve fibers rapidly undergo degeneration when the posterior pituitary is isolated and cut into small pieces, the disappearance of the

neurosecretory material in tissue culture may be expected. After 7 days in culture this phenomenon is still more pronounced, since by that time all nervous elements have degenerated; no granules or droplets of material stainable with chrome hematoxylin are visible. On the other hand, because of the disappearance of the nervous network, the cellular elements of the posterior lobe can now be studied under optimal conditions. The outlines of the cells are clearly visible and since the cultures can be examined as total mounts, the cells can be studied in their entirety. Figure I, taken from a moving picture record of a

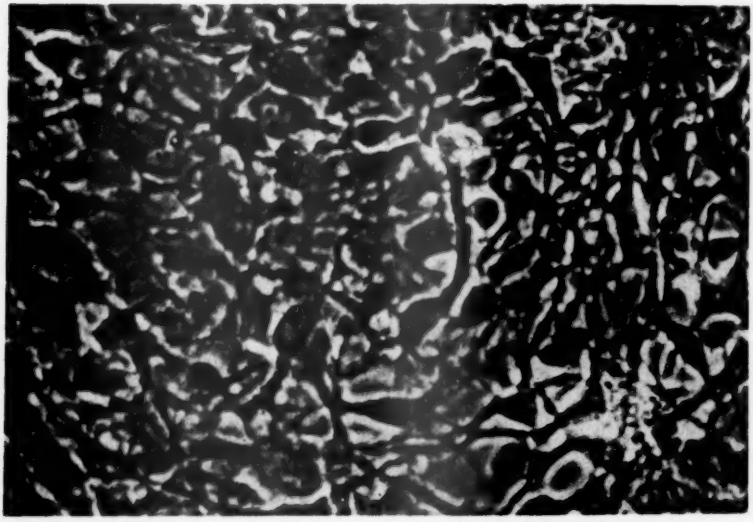

FIG. I. Glia cells in a six-day-old culture of rat posterior pituitary. Phase contrast, moving picture record. x 200.

6-day-old culture (phase contrast), shows glia cells of the posterior lobe which are connected with each other by their processes; they resemble protoplasmic astrocytes. In the living state, the cells show very characteristic movements; particularly remarkable is a shaking motion which the cells execute while they are suspended by their processes. The glia cells migrate at a very much slower rate than the spindle shaped connective tissue cells. The glia cells are, therefore, found mainly in and around

the original explant area, whereas the majority of the spindle shaped cells lies in the periphery forming at this time a ring around the inner area of the whole culture. The nuclei of the glia cells are round or oblong, their chromatin structure is distinct, their nucleoli are frequently paired. Since the same type of glia cells is also seen in cultures of brain tissue, the question arises whether the neurohypophyseal glia cells deserve to be singled out as "pituicytes" (Bucy, 1930) or whether no distinction should be made between them and the glia of other parts of the central nervous system. Shanklin (1944) mentions the numerous connections among the "pituicytes" as the "major difference between them, astrocytes and oligodendroglia," whereas Vazquez-Lopez (1942) comes to the conclusion that "there is no specific type of neuroglia peculiar to the neurohypophysis." Our observations of the living cells in tissue cultures of posterior lobe and brain support the point of view of Vazquez-Lopez, since the protoplasmic astrocytes of other parts of the brain show the same arrangement of connection with each other.

From the disappearance of the stainable neurosecretory substance which, by the way, could be observed only in our fixed and stained preparations, but not in living cultures studied with the phase contrast microscope, one had to conclude that the hormones also had disappeared. Extracts were prepared therefore in the usual manner from the cultured material as well as from the culture media. The hormone content was compared with that of the posterior lobe of normal animals. At point 6 of the smoke curve in figure II the vasopressor effect of an extract of one posterior pituitary of a normal rat injected intravenously in a decapitated cat is shown. By comparison, the extract given at point 5 taken from the entire cultured material of a posterior lobe of a normal rat grown *in vitro* for 4 days shows a considerably smaller effect. After 7 days *in vitro* the extract of a whole posterior pituitary (injection number 4) has practically no effect on the blood pressure of the decapitated cat. These findings are in agreement with the disappearance of the stainable neurosecretory material after 7 days of culture. The conclusion seems justified that the original amount of the hormones carried with the tissue into the culture medium was gradually decreased by inactivation. It seems to be clear that there is no new formation of hormone in the explanted tissue. It was never possible to de-

tect any hormonal activity in the culture medium outside of the culture itself.

Bioassays for oxytocic and antidiuretic hormones were not made from 4-day-old cultures. In the 7-day-old cultures these two fractions could not be detected, and no hormonal activity whatsoever was found in cultures of rat or dog material after the 10th day *in vitro*. The difference in hormone content between the uncultured and the cultured material was so striking that quantitative determinations were unnecessary. There is evidently no hormone formation by posterior lobe cells in tissue culture.

In addition to these experiments with posterior lobe tissue of normal animals, experiments were made with cultures derived from pituitaries depleted of neurosecretory substance. It has been shown by several authors (Ortmann, 1951; Hild, 1951; Hild and Zetler, 1953b; Leveque and Scharrer, 1953) that the posterior lobe can be depleted almost entirely of stainable neurosecretory material and at the same time of hormones (Simon and Kardos, 1934; Hickey, Hare, and Hare, 1941; Hild and Zetler, 1953b). A quick way to deplete a rat's posterior lobe is by the intraperitoneal injection of a 5 per cent NaCl-solution (5 cc. per 100 gm. of body weight daily for 4 days; Ortmann, 1951). As shown in the injection at number 7 in figure II, the hormone

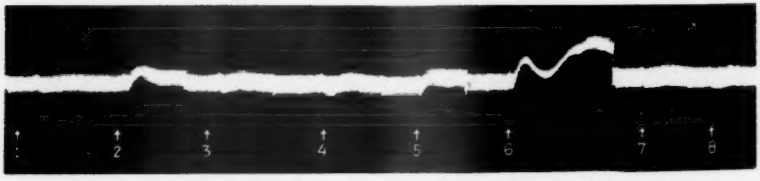

FIG. II. Blood pressure record of a decapitated cat (2, 5 kg.).

1. Extract of a four-day-old culture of one posterior pituitary of a dehydrated rat.
2. Standard = 0.05 I.U. Pituitrin per kg. of body weight.
3. Extract of the culture fluid from the same culture as used in No. 5.
4. Extract of a seven-day-old culture of one posterior pituitary of a normal rat.
5. Extract of a four-day-old culture of one posterior pituitary of a normal rat.
6. Control extract of one posterior pituitary of a normal rat without having been in culture.
7. Control extract of one posterior pituitary of a dehydrated rat without having been in culture.
8. Extract of two seven-day-old cultures of posterior pituitaries of dehydrated rats.

content of a rat's posterior lobe dehydrated in this way is almost zero. The numbers 1 and 8 of the same curve show the inactivity

of extracts prepared from 4- and 7-day-old cultures set up from such depleted tissue.

Bioassays were performed with material which had been cultured *in vitro* for up to 32 days. In no case was hormonal activity detected after the 10th day. Posterior lobe cells are, therefore, unable to produce hormones. This result confirms our concept about the hypothalamic origin of those hormones as expressed in earlier papers (Hild and Zetler, 1951–1953). The stored hormones introduced together with normal posterior lobe tissue into the culture medium can be shown to be present there for a short time until they are inactivated or destroyed in the culture medium. If the explanted tissue does not contain hormones as in posterior lobes obtained from dehydrated animals, no hormonal activity of the cultured tissue can be demonstrated at any time.

Older cultures proved very suitable for the study of the behavior of living posterior lobe cells. First it might be mentioned again, that in no instance and at no time could there be observed a morphological difference or a difference in the movements between the protoplasmic astrocytes from hypothalamus and cerebellum and the glia cells of the posterior lobe. There is also no significant difference in the behavior of cells from normal pituitaries and those from depleted organs. The moving picture records show perhaps a slightly higher migratory activity of the cells from dehydrated animals, but their general appearance is the same.

As the cultures grow older, two observations of general interest have been made: as the connective tissue cells (spindle cells) finally seem to reach a stage of fairly high differentiation, the cells derived from the glial elements undergo a more or less pronounced dedifferentiation; or better, they show peculiar signs of adaptation to the new conditions of life. Fixed and stained cultures as well as those observed in the living state with phase contrast, show in the zone of outgrowth a remarkable number of fibrous elements, which are spread out among the spindle cells in a manner resembling the arrangement of the connective tissue fibers in the posterior lobe as seen in fixed and stained sections of the organ (Romeis, 1940). On the other hand, the glia cells gradually lose their processes the farther they migrate from the center of the explant toward the periphery. In the moving

picture records it is often seen that the cell processes are withdrawn, and the original connections between the glia cells are lost. The individual cells preserve for a short time a shape which resembles the original polymorph type, but the processes are shorter and less numerous. Finally the processes are withdrawn entirely and the cytoplasm spreads out to form membranes. The cells are then hard to distinguish from epithelial cells. Beyond this, they can form a kind of an epithelial layer which, of course, is not identical with a true epithelial sheet. Without the evidence gained from moving picture records, it would be impossible to recognize these epitheloid cells as the same elements which only a short time ago had been multibranched cells of the protoplasmic astrocyte type. The same changes as to form and connections could be observed in glia cells in cultures of the hypothalamus and of cerebellum, a fact which speaks also in favor of the similarity between the glia cells of the hypophysis and those of the brain.

Discussion. The results of the experiments reported here support the new concept of the hypothalamic origin of the so-called posterior lobe hormones. Evidently the hormones cannot be elaborated by the posterior lobe cells. The fact that Geiling and Lewis (1935) and Anderson and Haymaker (1935–36) could demonstrate the presence of these hormones in tissue cultures of posterior lobe cannot be explained by the ability of the cells to produce hormones, but it is an expression of the temporary maintenance of the hormones, which have been carried with the tissue into the culture medium. The positive outcome of the bioassays merely indicates that the hormones had not yet been completely inactivated at the time (up to 6 days) when the cultures were tested. Although Geiling and Lewis write, that their cultures were observed for from 3 to 50 days, the blood pressure records in their paper show a vasopressor activity only in cultures tested at the 2nd, 5th, and 6th day of cultivation. It is possible, however, that the hormone content in their material was somewhat higher than in the experiments reported here because the authors did not use the roller tube technique, but instead the hanging drop method, in which the possibility of washing the hormones out is not as high as in roller tubes. The authors themselves do not emphasize their results so much, but Griffiths (1938), without having made cultures and bioassays himself,

interprets the results of the experiments of Geiling and Lewis as an indication of the formation of hormones by the "pituicytes." He comes to the conclusion, that since no elements other than pituicytes are maintained in tissue culture, the nerve fibers undergoing degeneration, the pituicytes are responsible for the presence of hormones in the cultures. This concept, however, is contradicted by the explanation of Ranson and his coworkers regarding the mechanisms causing diabetes insipidus in their experimental animals. The investigators of this group are convinced that the "pituicytes" have a secretomotor innervation by the fibers of the supraoptico-hypophyseal tract. They explain the diabetes insipidus occurring after transection of the hypophyseal stalk as due to an interruption of the innervation of the "pituicytes," which thus are said to become incapable of hormone production. If this were correct, hormone production in tissue culture should equally be impossible because there too the posterior pituitary cells are deprived of their supposed innervation. Beyond this, Green (1947) states that "from an anatomical standpoint it would still be necessary to establish that neural connections exist between the tractus hypophyseus and the pituicytes." Concerning the hormone content of tissue cultures, more exact observations are reported by Anderson and Haymaker (1935–36), who compared the amount of hormones found in untreated normal posterior lobes with that present in posterior and intermediate lobe tissues grown *in vitro*. They found an increase of melanophore hormone in the cultured material of intermediate lobe, a fact which can be interpreted as a true formation of intermedin in the cultures *in vitro*. The posterior lobe hormones were not found to be increased in their experiments.

The glia cells of the posterior lobe do not differ in any particular respect from glia cells in the brain and they do not show cytological signs of secretory activity. The granules described by Gersch (1937, 1939), Gersh and Brooks (1941), and Wang (1938) in the glia cells of the posterior lobe are evidently not secretory granules. Gersh reported an increase of the number of these inclusions in the "pituicytes" as a result of dehydration. It is difficult to understand why inclusions, if they have anything to do with the antidiuretic hormone, should be increased in dehydrated animals, *i.e.*, at a time when the content of anti-

diuretic hormone in the posterior lobe is very low (Simon and Kardos, 1934). In addition, the observations of Gersh were not confirmed by Hickey, Hare, and Hare (1941), DeRobertis and Primavesi (1942), and Desclin (1947). The granules described by Gersh and Wang were possibly gliosomes.

It is evident from the investigations of Rasmussen (1938) that the number of nerve fibers entering the neurohypophysis is very much higher than the number of cells which they are said to innervate according to Ranson and his school. Beyond this it never has been proved that the relations of the nerve fibers to the cells of the posterior lobe resemble what is generally considered as innervation (Green, 1947). The statement of Fisher (1937) according to which the "pituicytes" undergo degeneration after denervation requires reinvestigation. Fisher regards as a sign of degeneration the withdrawal of the processes of the "pituicytes." This disappearance of the processes and the hypercellularity of the posterior lobe after stalk section can be explained differently. It is known that after stalk section the nerve fibers in the posterior lobe disappear. Since they represent a large part of the tissue of the neurohypophysis the remaining cells draw closer together after the disappearance of the nervous component. Also the processes of the cells which form a network will become shorter as the cells lie closer together. The "hypercellularity" should therefore be regarded as a relative one without any actual increase of the number of the cells; because of lying closer together there appear in a field under the microscope at a given magnification more cells than in a section of normal tissue.

It has already been stated by Vazquez-Lopez (1942) that he could not find any significant difference between the morphological characteristics of glia cells in the neurohypophysis and those in the brain. This statement can be confirmed by the observations reported here on the glia cells of the posterior lobe, hypothalamus, and cerebellum. In tissue culture, where we have the advantage of observing the cells in the living state, the glia cells derived from posterior lobe have the same morphological characteristics as protoplasmic astrocytes derived from various parts of the brain. In the moving picture records, it is impossible to distinguish between the cells of one or another region. Their morphology is the same and the very typical shaking

movements of the cell body are visible in all cells of the protoplasmic astrocyte type. This motion is different from the pulsations of the cell body of oligodendrocytes which Pomerat (1951) described as well as Canti, Bland, and Russell (1935). The multibranched glia cells of all parts of the brain can give up their connections with other cells, and as separate elements they can spread out to become epitheloid cells while withdrawing their processes. This phenomenon is always observed in older cultures, where the nerve fibers have already disappeared, the presence of which is probably essential for the maintenance of the conventional form of glia cells. On the other hand, in living cultures as well as in fixed and stained ones of the posterior lobe, no cells were observed which resemble the type of the fibrous astrocyte, which can be often seen in cultures of brain tissue. Oligodendroglia cells are sometimes observed in cultures of posterior lobe; macrophages, which according to the investigations of Costero (1930) are probably identical with microglia, are a very common observation. The main part of the glia in posterior lobe of rat and dog, however, seems to be identical with the protoplasmic astrocyte. The only animal in which the posterior lobe glia seems to have a peculiar morphological shape is the ox, in which animal Bucy (1930) described this cell type and gave it the name "pituicyte." The "pituicytes" described by other authors in the posterior lobes of various animals and man, differ from the ox "pituicytes" to a considerable degree. Therefore, this name for a supposedly specific cell type of the posterior lobe should be abandoned or used only with the understanding that it designates nothing more than glia cells present in the posterior lobe.

Summary. Posterior pituitary tissue *in vitro* as well as in the living organism is unable to produce hormones. The hormones, normally stored in this tissue and transferred with the explants into the culture medium, are present in the tissue *in vitro* for several days. In the course of degeneration and disappearance of the nerve fibers in the cultures, the hormones together with their stainable carrier substance also disappear. If the posterior lobe is depleted of hormones prior to being explanted, as in dehydrated animals, the cultures never yield hormones.

As seen in fixed and stained cultures from 4 to 32 days of age as well as in moving picture records of living cultures, the glia

cells of the posterior lobe do not differ morphologically from ordinary glia in the brain. The name "pituicyte," designating a special glandular cell type of the posterior lobe should therefore be abandoned.

The results of the experiments here reported speak clearly in favor of the concept of the hypothalamic origin of the "posterior lobe hormones."

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THE CURRENT STATUS OF RABIES IN TEXAS*

J. V. Irons, R. B. Eads, Thelma Sullivan, and J. E. Grimes

Rabies has long been prevalent in Texas as well as the remainder of the United States and since the creation of the Texas Pasteur Institute in 1904 many species of wild and domestic animals have been found infected. Skunks of the genus *Spilogale* historically have been referred to as "hydrophobia cats" and were said to have caused deaths among cowboys along the Chisholm Trail a hundred years ago. Twenty-nine human deaths from rabies have been recorded in Texas in the last 10 years.

Pasteur treatments were given at the Texas Pasteur Institute until 1928, at which time the Institute was absorbed by the Bureau of Laboratories, Texas State Health Department. Phenolized rabies vaccine (Semple) supplanted the Pasteur treatments in Austin in 1933 when state-wide distribution of the vaccine to City and County Health Officers was begun.

In 1953, 2,675 14-injection human rabies treatments were prepared and furnished physicians for treatment of the people of Texas by the State Health Department. In view of the time and money being spent on this disease, it is felt that an evaluation of the effectiveness of our present control measures would be appropriate. This can perhaps be best done by an appraisal of the current infection level of rabies in the various epidemiological groups.

Rabies in Domestic Animals

Dog Rabies. Canine, as well as other animal rabies in Texas, occurs principally in the eastern half of the state at the present time, excepting certain border counties. Historically, the state's population has been largely centered in this same area. In recent years there has been some westward shift in population although it has not as yet been reflected in an expansion of the canine rabies endemic area. As shown in table 1, dog rabies was demonstrated in 86 counties in 1944 and in 81 counties 10 years later

* From the Texas State Department of Health, Austin. Received for publication June 5, 1954.

(1953). In 1944 there were 863 positive dogs diagnosed by this Laboratory, while 1,029 positive heads were diagnosed in 1953.

TABLE 1

Texas Rabies Distribution for the Ten Year Period 1944–1953

Year	No. Pos. Dog	No. Counties Involved	No. Pos. Fox	No. Counties Involved	No. Pos. Skunk	No. Counties Involved	No. Pos. Bovine	No. Counties Involved
1944	863	86	3	1	1	1	24	14
1945	763	92	0	0	1	1	17	11
1946	1,004	94	16	6	15	13	29	21
1947	956	101	40	13	7	6	25	20
1948	1,203	102	75	16	73	17	48	27
1949	712	77	129	27	44	29	43	24
1950	863	78	160	26	43	29	61	32
1951	970	73	181	37	58	31	61	28
1952	1,009	75	161	42	74	37	51	33
1953	1,029	81	150	44	49	20	53	33

Of the 3,059 dog heads received by this Department in 1953, 1,029 were positive, 1,914 were negative, and 116 were decomposed and unsuitable for testing. Thus, approximately one out of three cases of suspected dog rabies received Laboratory confirmation. The largest number of positive dogs were submitted in January (112) and the smallest number in June (61).

These data suggest that current rabies control measures are proving ineffectual in eliminating rabies from the dog population in this state, although morbidity statistics indicate that they have been effective in preventing human cases of the disease. A potentially explosive situation exists as long as there is a high level of rabies in dogs, however.

In Texas the control or abatement of rabies for the most part is in the hands of municipal authorities. A statewide rabies control act was introduced in the 1953 Legislature but it was not enacted into law. In 1954 a bill was passed authorizing the Commissioners Court of any county having a population of 800,000 or more to regulate dog populations outside the boundaries of incorporated cities during a rabies epidemic. However, this legislation is applicable only to Harris County.

As a result of the lack of coordination, remedial measures have not been uniformly or consistently applied. Under the stimulus of a sharp increase in animal rabies, most cities have enforced existing ordinances providing for compulsory vaccina-

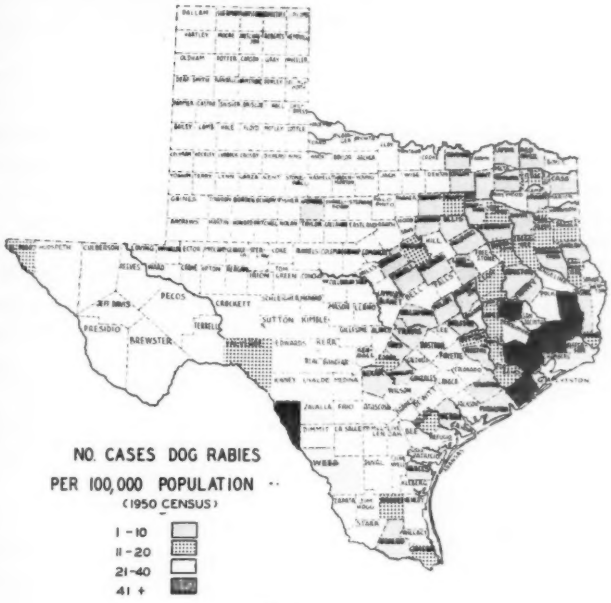

FIGURE I

tion and the collection of stray animals. With a decline in incidence, the municipal ordinances have frequently been only perfunctorily enforced. Thus, although there has been a considerable fluctuation in the number of rabid animals in the various Texas cities, the overall state level of infection in dogs has remained at a consistently high level.

Cat Rabies. In point of numbers, cat heads submitted for rabies examination are second only to dogs. In 1953 there was approximately 1 cat head received for every 3 dog heads. However, the percentage of positive cat heads was much lower than that of the dogs. Only 68 cats out of 1,039 heads, or 6.5 per cent, were demonstrated to be positive. In past years the percentage of cat heads received which were positive has been as low as 2 per cent.

It is apparent that the infection level of rabies in cats is considerably lower than in dogs; this is no doubt due to the fact that they spend a large part of their time indoors and to their

ability to avoid rabid animals when outdoors. It is felt that controlling rabies in dogs will practically eliminate the disease in cats.

Bovine Rabies. One hundred and seventy-three cattle heads were submitted to this Department for rabies examination in 1953 and 53 of them were positive. The number of animals involved were too few to be significant in themselves; but, coupled with field investigations, appear to justify certain generalizations.

During the last 10 years there has been a steady increase in the number of counties reporting cattle rabies, which parallels the spread of rabies in fox. However, our records indicate that there was an appreciable amount of cattle rabies in 1944 and 1945, when fox rabies was rare. This indicates that the dog, skunk or other animals are also factors in the spread of the disease to cattle. Emphasizing this point is the fact that counties along the Mexican border in which fox rabies is unknown report cattle rabies.

The cattle submitted to this Department are usually dairy animals, or others in close contact with human beings. As a rabid cow experiences difficulty in swallowing, the owner frequently sticks his arm down the animal's throat to search for obstructions. When a veterinarian is called and a clinical diagnosis of rabies made, the cow head is usually submitted for laboratory confirmation, prior to the owner seeking anti-rabies treatment. These animals are much more closely associated with dogs than with fox. However, range animals are frequently lost to rabid fox.

Rabies in Wild Animals

Fox Rabies. Texas has experienced a marked increase in wildlife rabies in recent years, particularly in the fox (Young and Irons, 1953). Prior to 1946 rabies was seldom reported in these animals in Texas, but for the last few years the disease has occurred in epizootic proportion in the gray fox *(Urocyon cinereoargenteus)* population. As may be seen in table 1, only 3 rabid fox were received by this Department in 1944 and these were from a single county; none were seen in 1945 and 16 from 6 counties were received in 1946. In contrast, 150 rabid fox were taken from 44 counties in 1953 (Fig. II).

In 1953, as in other years, the largest number of infected fox were received in November, December, January, February, March, and April. Little fox rabies is encountered during the summer months, coincident with the scattering of the animals to rear their young. Only 17 per cent of the positive fox examined in 1953 were received during the 6 months from May through October.

The actual rabies infection level now existing in fox is simply a matter of conjecture. Reliable estimates of the gray fox populations are lacking. The fox is known to be widely distributed throughout the entire state. Formerly, the animal has been scarce in the brush country south and west of Austin and San Antonio. However, farmers and ranchers are reporting an in-

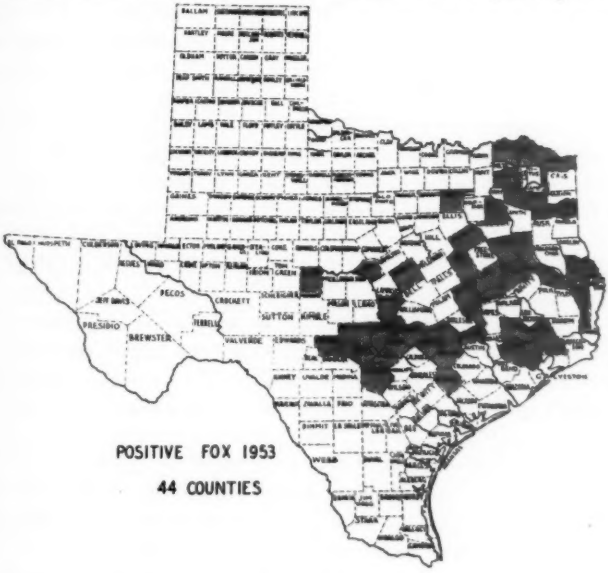

FIGURE II

crease in gray fox in this area, which may help to explain the westward spread of fox rabies.

Eighty-eight per cent of the fox submitted to this Department in 1953 were proven rabid by laboratory tests. This, of course, is by no means random population sampling. It is no small job

to cut off a fox head and pack it in a suitable container in ice for shipment to this Department. This is seldom undertaken unless the animal has invaded a premise during daylight hours and attacked dogs, domestic animals or people. Fox demonstrating so drastically altered behavior patterns are likely to be rabid.

Obviously, the positive fox seen by this Laboratory in 1953 represented but a small fraction of the total. It was not uncommon to find dead animals along wooded watercourses in counties where the disease occurred in epizootic proportions. This disease is self-limiting within a given area and in time will deplete the fox below the population level necessary to sustain infection. However, this natural control is slow and is not effective on a state-wide basis. Even though rabies will burn itself out in a county within a year or two, the disease will simply spread to other counties with high fox populations or to other species of animals. During the epizootics, large numbers of domestic animals are lost to rabid fox.

The only feasible method of control would appear to be wildlife thinning, with abatement and not extirpation the objective. In this state, fox reduction has been attempted by means of bounties and cooperative voluntary fox reduction induced by public health education. It is apparent that these efforts have not been successful since fox rabies has not been contained but has continued to spread.

In 1947, the Texas Legislature passed House Bill 537 which appropriated $15,000 to be used in paying $2.00 bounties for each fox killed in counties certified by the State Health Officer to have sufficient rabies in fox to be a menace to public health. Since some 18 East Texas Counties were in this category, this money was expended by 1949, without any appreciable effect on either the fox population or the overall rabies situation. In view of the tremendous number of fox involved, a bounty system does not appear to be economically feasible.

County fox abatement campaigns organized by this Department have been fairly successful in controlling rabies in local fox populations. Through cooperative efforts of the Fox Hunters' Association, County Agents, Civic Organizations, farmers and ranchers, fox populations have repeatedly been lowered sufficiently to control local epizootics of fox rabies. However, this public support is seldom forthcoming unless the disease is al-

ready rampant in fox and domestic animals are being lost. It follows that no permanent control is realized from fox thinning in epizootic counties if the disease spreads to fox in adjacent counties.

It is felt that the solution to the wildlife rabies problem is the employment, on a permanent basis, of skilled predatory animal trappers by either the State or Federal Government to insure that fox and skunk populations are kept below the level necessary to support rabies in epizootic proportions. Federal predatory animal trappers have been very effective in controlling coyotes and wolves and the same system should be applicable to fox abatement.

Skunk Rabies. Skunk rabies in Texas antedates written records. The disease has been so prevalent in these animals that it has been suspected that a carrier state might exist in the skunk. However, this theory has not been substantiated; rabies is apparently a fulminating disease in skunks as it is in all other mammals, with the exception of bats.

The number of positive skunks received by this Department for the last 10 years reached a peak in 1952 and decreased slightly in 1953. In 1944 a single positive skunk was received; by 1949 the number had reached 44 animals from 29 counties. The 1952 peak was 74 positive skunks submitted from 37 counties. Seasonal fluctuation in incidence of skunk rabies is not pronounced as is the case with fox rabies. In 1953 the largest number of rabid skunks was received in May (a total of 12) and in January (a total of 7). The remainder of the 49 positive skunks seen in 1953 were distributed fairly evenly throughout the other months.

As with fox, these data are not quantitative, and are merely suggestive of the extent of rabies in skunks. Sixty-seven per cent of the skunks received were positive; but the animals were usually not submitted unless they had bitten dogs, other animals or human beings.

Our experience has been that there is no justification for the popular assumption that there is an appreciable variation in skunk species susceptibility. The term "hydrophobia cat" has been applied to the several species of small, spotted skunks, *Spilogale* spp. However, over 80 per cent of the rabid skunks confirmed by this Department in 1953 were the striped skunk,

Mephitis mephitis. Essentially, this means that the skunk species in Texas in 1953 were about equally infected, since striped skunks are more numerous than the spotted skunks.

Field investigations have indicated that skunks are of considerable import in the epidemiology of dog rabies. In Tarrant County in 1951, there were 20 dogs which were diagnosed as being rabid in the laboratory. All were from rural areas and 18 of them had histories of fighting with skunks. Unvaccinated dogs in urban areas are constantly endangered by dogs from rural areas which have contracted the disease from skunks. This situation emphasizes the need for statewide rabies control which would provide for the vaccination of rural as well as urban dogs.

Efforts to control rabies in skunks have been limited to educational activities designed to familiarize the public with the various facets of the problem. Emphasis has been placed on the need for vaccination of rural dogs and for reduction of skunk populations. It is felt that, as with fox, the most efficient method of controlling skunk would be the utilization of permanently employed predatory animal trappers.

Bat Rabies. The isolation by Venters, *et al.* (1954), of rabies virus from insectivorous bats in Florida has heightened interest in these flying mammals as possible reservoirs of the disease agent. Bats are of particular concern to public health personnel in the states bordering Mexico. Vampire bats, *Desmodus rotundus*, have been found some 150 miles from the Texas border.

In Mexico a highly fatal paralytic disease of cattle has been prevalent for many years, but the etiological agent and its origin has been but recently known. A common Spanish name for the disease is derriengue. In 1932 Dr. A. Tellez Giron isolated virus from cattle which, in symptomalogy and pathology, resembled rabies. Dr. Ten Broeck of the Rockefeller Institute obtained a cattle strain of virus in 1943 which proved to be antigenically related to known strains of rabies.

Johnson (1948) conducted field investigations in the Mexican states of Michoacan and Jalisco. Seven localities where livestock had recently died from derriengue were studied and vampire bats were found in the vicinity of each focus of infection. Bat salivary glands and brains from one of the 7 localities proved to be positive. The virus was shown to be closely related to

known strains of rabies virus by cross-neutralization, cross-complement fixation, and cross-protection tests.

Derriengue or cattle rabies transmitted by vampire bats has been known in the Pacific Coast states of Mexico for at least 50 years. In recent years the disease has spread until it has now been reported in a total of 22 of the 32 states and territories of Mexico. Several outbreaks of rabies have occurred in Mexico in human beings following attacks by vampire bats.

Since vampire bats are tropical species, they are not likely to become established in the United States. However, some Texas bats may migrate into Mexico during the winter months and interspecies spread of the virus is an ever present possibility.

An investigation into the rabies carrying propensities of the Texas insectivorous bats is being conducted by this Department. A preliminary report of this work has been made by Sullivan, *et al.* (1954). Collections of bats are being made from all habitats, particularly in those areas in which wildlife rabies occurs. Over 400 bats have been collected to date. The species tested have included *Tadarida mexicana*, *Myotis velifer*, and *Pipistrellus subflavus*.

From the bats tested, 3 strains of virus pathogenic for mice have been isolated. The first isolation was from a brain pool composed of 1 *T. mexicana* brain and 1 *M. velifer* brain. Both bats were taken at Camp Bullis in Bexar County. The agent was identified as a strain of rabies virus on the basis of incubation period and symptoms in mice and the presence of intracytoplasmic inclusion bodies indistinguishable from Negri bodies in the mice brains; finally, mice were protected against this virus with anti-rabies serum.

A second strain of rabies virus has been isolated from a *T. mexicana* collected in Travis County and a third from a *T. mexicana* taken in Bexar County.

This survey is too incomplete to formulate any conclusion relative to the rabies infection level in Texas bat populations. However, millions of free-tailed bats, *T. mexicana*, inhabit caves and buildings throughout the state. Four of the biggest bat colonies in the United States are present in Ney Cave (Medina County), Bracken Cave (Comal County), Frio Cave (Uvalde County), and Devil's Sinkhole (Edwards County). All four caves are in central Texas. Two of them far surpass the huge

bat population in Carlsbad Caverns. Numerous other species, both colonial and solitary, are common. The epidemiological implications of the findings of rabies virus in those few insectivorous bats are not clearcut. This Department receives few reports of bats attacking human beings, though it sometimes does occur.

Miscellaneous Rabies

Miscellaneous carnivorous animals found to be positive in 1953 included 3 wolves, 2 raccoons, 1 coyote, and 1 bobcat. Although they are common carnivores, the raccoon and bobcat are seldom found to be rabid. Evidently their tree climbing ability enables them to avoid exposure to the disease. Illustrative of this point is the fact that we have never seen a rabid ring-tailed cat, *Bassariscus astutus*, although this arboreal animal is abundant over much of Texas. Four of these animals were tested in 1953, after they had bitten human beings, and were found to be negative.

Although this Laboratory receives numerous rodents for examination, natural infection with the rabies virus is apparently quite rare. During 1953, negative rodents examined included 48 domestic rabbits, 18 rats, 5 mice, 77 squirrels, 3 hamsters, 2 gophers, and 1 prairie dog.

Domestic animals other than cattle are occasionally infected with rabies. In 1953, 3 horse heads were received and one was shown to be rabid. One sheep and 7 goats were submitted during the same period and 3 of the goats were rabid. Fourteen hogs were examined in 1953 but none of them were found to be rabid. Occasionally rabid hogs are seen; one was diagnosed in 1952.

Summary

Data obtained by the Texas State Department of Health Bureau of Laboratories are presented to show that from the standpoint of human mortality rabies is an inconsequential disease. Only 29 human cases have been reported by Texas physicians from 1944 through 1953. However, the prevalence of rabies in dogs, fox, skunks, cattle, and its recently demonstrated presence in insectivorous bats necessitates the expenditure of con-

stant effort to keep down human exposure. In Texas rabies control is in the hands of municipal authorities and little progress is being made in suppressing the disease in the various animal reservoirs. A coordinated state-wide control program is urgently needed.

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CHEMOTHERAPY EXPERIMENTS WITH PLANT EXTRACTS AND TRANSPLANTABLE TUMORS*

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In an earlier paper (Taylor *et al.*, 1952) the results of screening plant extracts was reported. This sequel presents the results of another two years' work.

Experimental Procedures

Preparation of Extracts. Plant material was processed as soon as possible after collection. Some was stored in a deep freeze unit for later use in case the original material produced some positive results. In a few cases material was freeze-dried and stored at room temperature (27° C.).

The material was prepared as previously reported (Taylor *et al.*, 1952). The first extracts were as follows: aqueous pH 6.0, aqueous pH 7.0, aqueous pH 8.0, acetone, and absolute alcohol. Juice was expressed by means of a Carver hydraulic laboratory press from some of the more succulent plants and the resulting liquid sterilized by filtration. *Cucurbita foetidissima* H. B. K. (Gourd vine) was ground and the liquid obtained was sterilized for experimentation.

Sterility was obtained in all instances by cold filtration. A Selas No. 02 bacterial filter and a Corning ultra-fine glass were employed. The majority of plant extracts pass through either, but some extracts only go through one and not the other.

Methods of Testing. After testing the extracts for sterility, toxicity tests were conducted with eggs and mice. The material was introduced into non-tumor eggs over the blood vessels of the area vasculosa of the chick embryos on the eleventh day of incubation. Chick embryos were removed and weighed on the sixteenth day of incubation (Taylor *et al.*, 1952).

Toxicity tests were conducted with white mice in order to determine the tolerated dosage for tumor-bearing mice. These mice were injected with 3 mg. daily for five consecutive days. Sterile triple distilled water was the medium used to dilute the extract and for injection.

Tumor-bearing eggs were prepared as previously reported (Taylor *et al.*, 1953). The essentials of the method are a yolk sac inoculation through the air sac of embryonated eggs on the fourth day of incubation with a saline suspension of dispersed tumor tissue. Eggs were implanted with a C_3H mouse mammary carcinoma which grows relatively uniformly in each inoculated egg. This tumor, implanted in the eggs by the yolk sac method, attains a size of 0.2 mm. eight days after inoculation on the twelfth day of incuba-

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tion. Very rapid growth takes place between the twelfth and thirteenth days and the tumor doubles in size.

Extracts were introduced over the area vasculosa of twelve-day tumor bearing eggs by a method described elsewhere (Taylor, 1953). The absorption (Taylor, 1952) was made to proceed slower by the use of cottonseed oil as a suspending medium. Two parts oil to one part extract was used, or where a higher dosage was believed necessary, one part oil to one part extract.

Embryos and tumors were harvested on the thirteenth day and the experimental groups compared with the controls. Two different methods were used with transplantable tumors in mice for the effect of the various extracts. The older method was described by Taylor (1952) and later by Bennett (1953).

A preliminary evaluation was obtained by introducing the test material into a saline suspension of dispersed tumor tissue and incubated one hour at 37° C. Control tubes received sterile distilled water but were treated in a similar manner to the other tubes. Evaluation was made by comparing the growth of implants from tubes containing the test material with growth of the control implants. The method eliminates a great many ineffective extracts.

Also the plant extracts were used against transplanted tumors. Mice of the dba strain, bearing transplants of a mammary carcinoma, were used in most of the tests. Some work was done with mice implanted with a sarcoma. The mammary carcinoma tumor was implanted eight days before the series of injections was scheduled to begin. It grew to about 1.5 to 2.0 gms. in fifteen days. If a dba sarcoma was to be used it was implanted three days before the series of injections was scheduled to start. It grew to an average size of about 3.0 gms. in ten days. The experimental animals received five subdermal injections on successive days at a site removed from the tumor. Cottonseed oil was used to suspend the extract in a ratio of one part oil to one part extract in order to slow the absorption. Control mice received an injection of triple distilled water in cottonseed oil at the same dilution.

Results

Since our last report, approximately 300 extracts have been prepared. A few have proved toxic and hence of little value as an inhibitor of tumor growth. Many extracts inhibit both tumor and embryo growth and therefore have little value in tumor chemotherapy. A small group inhibit the tumor growth and have little or no effect on the chick embryo.

The results shown in table 1 are the positive results obtained with yolk-sac cultivated tumors. *Anemone decapetala* Ard gave the best results. Results with transplanted tumors in mice are shown in tables 2 and 3.

The results obtained by placing the plant extract in with the suspension of tumor tissue and incubating are shown in table 2.

Three plants produced most of the positive results; *Parthenium hysterophorous* L, *Phoradendron flavescens* (Pursh) Nutt, and *Prosopis glandulosa* Torr. These extracts did not exhibit a great amount of toxicity toward mice. Several extracts of *Phoradendron flavescens* lowered the weight and survival rate.

TABLE 1

Plant Extracts which Inhibited the Growth of Egg Cultivated Mouse Mammary Tumor with Little or no Effect on its Embryo—24 Hour Test

Plant	Type of Extract	No. Expts.	No. Eggs	Dosage	Survival Control = 100	Avg. growth for test period C=100	
						Tumor	Embryo
Acalypha phleoides Cav.	Abs. Alcohol	1	28	.1 ml.	85	48	124
Anemone decapetala	Abs. Alcohol	1	45	1.5 mg.	138	7.5	116
Astragalus Wootoni	pH 7.0	1	27	.6 mg.	91	41	98
Cannabis sativa	pH 6.0	1	28	.6 mg.	80	28	101
Cucurbita foetidissima	Abs. Alcohol	1	45	1.5 mg.	97	18	110
Cucurbita foetidissima	Juice	1	28	.07 ml.	84	55	131
Cola de caballo	pH 6.0	1	21	.6 mg.	81	50	194
Conium maculatum	pH 7.0	1	44	1.5 mg.	119	48	94
Datura	Abs. Alcohol	1	29	.33 mg.	93	29	120
Liatris punctata	pH 6.0	1	29	1.0 mg.	93	52	84
Muskmelon	Acetone	1	32	1.5 mg.	169	20	100
Muskmelon	Acetone	1	32	1.5 mg.	113	33	115
Muskmelon	pH 8.0	1	30	1.5 mg.	177	56	127
Muskmelon	pH 6.0	1	30	1.5 mg.	118	56	100
Papaya	pH 8.0	1	26	.6 mg.	124	37.4	104
Prosopis glandulosa	pH 6.0	1	32	1.5 mg.	93	36	100
Prosopis glandulosa	Abs. Alcohol	1	24	.3 mg.	220	45.2	104.4
Prosopis glandulosa	Acetone	1	23	.2 mg.	152	11.1	109.5
Prosopis glandulosa	Abs. Alcohol	1	27	.1 mg.	102	22	105
Prosopis glandulosa	Abs. Alcohol	1	27	.06 mg.	145	45	118
Phoradendron flavescens	pH 6.0	1	24	.45 mg.	180	50	107
Purple Sage	Acetone	1	27	.6 mg.	117	31.6	127
Purple Sage	Abs. Alcohol	1	28	.6 mg.	149	56.3	120
Rhus glabra	pH 7.0 Boiled	1	28	1.0 mg.	97.6	34	130
Salvia sp.	pH 6.0	1	22	.1 ml.	109	38.7	144
Salvia sp.	pH 7.0	1	24	.07 ml.	87	59	85
Salvia Greggi	Acetone	1	28	.1 mg.	100	53	87

Amphyteryngium abstringens (Schlect) Schiede (Martinez, 1933) and Cola de caballo have been used by herbalists in Mexico for treating cancer. The names used are those supplied by the herbalist from whom we obtained specimens of the plants in the dried state.

TABLE 2

The Effect of Plant Extracts When Mixed with Saline Suspension of Tumor Tissue and Incubated One Hour at 37° C and Injected into DBA Mice

Plant	Type of Extract	mg. or ml. of Extract in 1 ml. of Suspension	Dosage Injected	At Autopsy Per cent Control Wt. C = 100	Per cent Tumor Growth C = 100	Type Tumor
Amphyteryngium abstringens	Abs. Alcohol	.1 ml.	.02 ml.	98	0	dba mouse mammary
Cola de caballo	pH 7.0	.1 ml.	.02 ml.	90	32	dba mouse mammary
Liatris punctata	pH 7.0	1.5 mg.	.3 mg.	96	27	dba mouse sarcoma
Liatris punctata	pH 8.0	1.5 mg.	.3 mg.	90	9	dba mouse sarcoma
Liatris punctata	pH 7.0	1.5 mg.	.3 mg.	87	0	dba mouse sarcoma
Liatris punctata	pH 7.0 boiled	1.5 mg.	.3 mg.	100	0	dba mouse mammary
Parthenium hysterophorus	Acetone	.1 mg.	.02 mg.	95	2	dba mouse sarcoma
Parthenium hysterophorus	Acetone	.1 mg.	.02 mg.	93	0	dba mouse mammary
Parthenium hysterophorus	pH 7.0	1.5 mg.	.3 mg.	96	8	dba mouse sarcoma
Parthenium hysterophorus	pH 7.0 boiled	1.5 mg.	.3 mg.	100	2	dba mouse sarcoma
Parthenium hysterophorus	pH 7.0	1.5 mg.	.3 mg.	107	0	dba mouse mammary
Parthenium hysterophorus	pH 7.0 boiled	1.5 mg.	.3 mg.	102	0	dba mouse mammary
Phoradendron flavescens	pH 8.0	.3 mg.	.6 mg.	108.4	0	dba mouse sarcoma
Phoradendron flavescens	pH 6.0	.5 mg.	.1 mg.	82.3	0	dba mouse sarcoma
Phoradendron flavescens	Acetone	.1 ml.	.02 ml.	101	21	dba mouse sarcoma
Phoradendron flavescens	pH 7.0	1.25 mg.	.25 mg.	88	0	dba mouse sarcoma
Phoradendron flavescens	pH 7.0	.01 ml.	.002 ml.	82	0	dba mouse sarcoma
Phoradendron flavescens	pH 7.0	.01 ml.	.002 ml.	86	0	dba mouse sarcoma
Phoradendron flavescens	pH 7.5	.01 ml.	.002 ml.	98	0	dba mouse sarcoma
Phoradendron flavescens	pH 6.65	.000335 ml.	.000067 ml.	103	0	dba mouse sarcoma
Phoradendron flavescens	Abs. Alcohol	.1 ml.	.02 ml.	97	0	dba mouse sarcoma
Phoradendron flavescens	Acetone	.1 ml.	.02 ml.	95	5	dba mouse sarcoma
Phoradendron flavescens	pH 7.0	.1 ml.	.02 ml.	90	0	dba mouse sarcoma
Phoradendron flavescens	pH 7.0	.1 ml.	.5 mg.	81	0	dba mouse sarcoma

TABLE 2—(Continued)

The Effect of Plant Extracts When Mixed with Saline Suspension of Tumor Tissue and Incubated One Hour at 37° C and Injected into DBA Mice

Plant	Type of Extract	mg. or ml. of Extract in 1 ml. of Suspension	Dosage Injected	At Autopsy Per cent Control Wt. C=100	Per cent Tumor Growth C=100	Type Tumor
Piscaria sp.	Abs. Alcohol	.3 mg.	.6 mg.	91.4	19.8	dba mouse sarcoma
Prosopis glandulosa	Abs. Alcohol	.3 mg.	.6 mg.	95	0	dba mouse sarcoma
Prosopis glandulosa	pH 8.0	.3 mg.	.6 mg.	101	3	dba mouse sarcoma
Prosopis glandulosa	Acetone	.3 mg.	.6 mg.	96	9	dba mouse sarcoma
Prosopis glandulosa	Abs. Alcohol	.3 mg.	.6 mg.	92	12	dba mouse mammary
Prosopis glandulosa	Acetone	.3 mg.	.6 mg.	91	0	dba mouse sarcoma
Prosopis glandulosa	pH 8.0	.3 mg.	.6 mg.	94	20.4	dba mouse sarcoma
Prosopis glandulosa	Abs. Alcohol	.3 mg.	.6 mg.	81	13	dba mouse sarcoma
Prosopis glandulosa	Abs. Alcohol	.3 mg.	.6 mg.	91	0	dba mouse sarcoma
Prosopis glandulosa	pH 7.0 boiled	.3 mg.	.6 mg.	89	30	dba mouse sarcoma
Prosopis glandulosa	Abs. Alcohol	.3 mg.	.16 mg.	93	25	dba mouse sarcoma
Prosopis glandulosa	pH 6.9 boiled	.1 ml.	.02 ml.	101	21	dba mouse sarcoma
Prosopis glandulosa	Abs. Alcohol	.1 ml.	.02 ml.	104	15	dba mouse sarcoma
Ricinus communis L.	pH 6.9	.1 ml.	.02 ml.	106	0	dba mouse sarcoma
Vitis candicans	pH 6.0	.3 mg.	.6 mg.	95	2	dba mouse sarcoma
Vitis candicans	pH 8.0	.3 mg.	.6 mg.	90	0	dba mouse sarcoma
Vitis candicans	pH 8.0	.3 mg.	.6 mg.	93	13.2	dba mouse mammary
Yucca arkansana	pH 7.5	.1 ml.	.02 ml.	107	0	dba mouse sarcoma
Yucca arkansana	pH 7.5	.01 ml.	.002 ml.	99	12	dba mouse sarcoma
Yucca arkansana	pH 7.0	.1 ml.	.02 ml.	100	5	dba mouse sarcoma
Yucca arkansana	pH 7.5	.1 ml.	.02 ml.	99	19	dba mouse sarcoma
Yucca arkansana	pH 6.6	.1 ml.	.02 ml.	96	12.5	dba mouse sarcoma
Zephranthes texana	pH 6.6	.1 ml.	.02 ml.	107	0	dba mouse sarcoma

The results obtained with mice receiving the various types of tumor transplants and subsequently 5 injections are shown in table 3. The method was considered more rigorous and if the tumor growth was kept under 75 per cent of the control, the extract was considered as having some value.

Cucurbita foetidissima H. B. K. showed some inhibition when the expressed liquid from leaves and stems was used. *Liatris punctata* Hook inhibited tumor growth and the material must be fairly stable as it withstood boiling for fifteen minutes. The plant is a common one growing wild in this area. It is also noteworthy in that it has shown inhibition by all three methods of testing. The extracts are relatively non-toxic to mice. Mice were given five 3 mg. injections and the weight was not adversely effected in any case.

Phoradendron flavescens and *Prosopis glandulosa* inhibted tumor growth by all methods. However, certain extracts were extremely toxic to chick embryos and mice. The berries of *Phoradendron flavenscens* were especially toxic. *Prosopis glandulosa* was toxic at certain stages of growth.

Several plants which produced inhibition by two different methods of testing are listed in table 4.

Cucurbita foetidissima H. B. K. and a relative, muskmelon, produced some inhibition of tumor growth. *Parkinsonia aculeata* and *Prosopis glandulosa* are in the same family and have shown some inhibition.

The effectiveness of plant extracts may differ in association with such factors as soil and weather. Hence, the results obtained here may not be duplicated elsewhere. Plants producing negative results by our methods may give positive results by other approaches. The screening program is continuing and some plants which have given promising results will be investigated further. The plants used in the testing program are shown in table 5.

Summary. The results indicate that some plants contain substances which will inhibit the growth of tumors. Twenty-one plants showed some inhibition of the growth of yolk-sac cultivated tumor. *Anemone decapetala* was the most effective in the group. Thirteen plants inhibited tumor growth when tumor tissue and extract were mixed, incubated at 37° for one hour, and injected into mice. Extracts from several plants gave com-

TABLE 3

Positive Results Secured with Five Consecutive Injections of Plant Extracts in Mice with Transplanted Tumor

Plant	Type of Extract	Dosage	Per cent Body Wt. at Autopsy C=100	Per cent Tumor Growth C=100	Type Tumor
Allium helleri	pH 7.0 boiled	1.5 mg.	103	59.7	dba mouse mammary
Allium helleri	Acetone	3 mg.	105.6	71.2	dba mouse mammary
Anemone decapatala L.	pH 7.0 boiled	1.5 mg.	102	71	dba mouse sarcoma
Cucurbita foetidissima	Juice-leaves and stems	.1 ml.	100	70	dba mouse sarcoma
Cucurbita foetidissima	Juice-flowers	.1 ml.	96	68	dba mouse sarcoma
Cucurbita foetidissima	Juice-blooms	1.5 mg.	103	60	dba mouse mammary
Cucurbita foetidissima	pH 7.0	1.0 mg.	98	54	dba mouse mammary
Cucurbita foetidissima	Juice-leaves and stems	.1 ml.	87.6	31	dba mouse mammary
Cucurbita foetidissima	Juice Fl. and B.	.1 ml.	84.1	54	dba mouse mammary
Delphinium (Seeds.)	pH 6	1.5 mg.	94	56	dba mouse mammary
Gossypium sp.	pH 8.0	1.5 mg.	99.4	69	dba mouse mammary
Liatris punctata	pH 7.0	.5 mg.	99	60	dba mouse mammary
Liatris punctata	pH 7.0 boiled	1.5 mg.	116	65	dba mouse mammary
Liatris punctata	pH 8.0	1.0 mg.	98	34.4	dba mouse mammary
Liatris punctata	pH 7.0	1.0 mg.	98	43.9	dba mouse mammary
Liatris punctata	pH 7.0 boiled	1.0 mg.	99	36.7	dba mouse mammary
Melia azadarach	Abs. Alcohol	1.0 mg.	95	50	dba mouse mammary
Parkinsonia aculeata	pH 7.0 boiled	1.5 mg.	104	42	dba mouse mammary
Parkinsonia aculeata	Acetone	.5 mg.	99	58	dba mouse mammary
Phoradendron flavescens	pH 6.0	2.4 mg.	105	32	dba mouse mammary
Phoradendron flavescens	pH 7.0	2.4 mg.	97	50	dba mouse mammary
Prosopis glandulosa	pH 8.0	1.5 mg.	95	69	dba mouse mammary
Prosopis glandulosa	Abs. Alcohol	.5 mg.	94	35	C_3H mammary
Prosopis glandulosa	Abs. Alcohol	.5 mg.	94	56	C_3H mammary
Rhubarb root	Acetone	1.5 mg.	97	62	dba mouse mammary
Rhubarb root	Acetone	1.5 mg.	101	38.8	dba mouse mammary
Rhus glabra	pH 8.0	1.5 mg.	96	62	dba mouse mammary

TABLE 4

Plant	Methods
Anemone decapetala	Yolk-sac cultivated tumor Mouse with five injections
Cucurbita foetidissima	Yolk-sac cultivated tumor Mouse with five injections
Cola de caballo	Yolk-sac cultivated tumor Mouse by one injection
Parkinsonia aculeata	Yolk-sac cultivated tumor Mouse with five injections
Rhus glabra	Yolk-sac cultivated tumor Mouse with five injections
Vitis candicans	Yolk-sac cultivated tumor Mouse by one injection

TABLE 5

Plants Used in Tumor Tests

Acocotillo o feurogreco[1]	*Melia Azedarach* L.
Acolypha phleoides Cav.[1]	Muskmelon
Allium helleri Small	*Nicotiana glauca* Graham
Ambrosia aptera DC	Oleander sp.
Amphyteryngium abstringens[1] (Schlect) Schiede	Papaya sp.
Anemone decapetala Ard	*Parkinsonia aculeata* L.
Asclepias decumbens L.	*Parthenium hysterophorus* L.
Astragalus Earlei Greene[2]	*Phoradendron flavescens* (Pursh) Nutt.
Astragalus Wootoni Sheldon[2]	Piscaria sp.
Cannabis sativa L.	*Poinciana pulcherrima* L.
Chicorium intybus L.	*Prosopis glandulosa* Torr.
Cnidoscolus texanus (Muell. Arg) Small	Rhubarb root[4]
Cola de caballo[1]	*Rhus glabra* L.
Conium maculatum L[3]	*Ricinus communis* L.
Cooperia pedunculata Herb.	*Salvia farinacea* Benth.
Cucurbita foetidissima H. B. K.	*Salvia Greggi* A. Gray
Datura	Salvia sp.[1]
Delphinium sp.[4]	Sarsaprilla sp.[4]
Euphorbia marginata Pursh	Sensitive Plant
Gossypium sp.	*Sophora secundiflora* (Ortega) Lag.
Leucophyllum frutescens (Berl.) Johnston	Spearmint[4]
Liatris punctata Hook	*Tecoma radicans* (L) DC Seem
Lupinus texansis Hook	*Toxicodendron radicans* L.
	Verbena bipinnatifida Nutt.
	Vitis candicans Englem.
	Yucca arkansana Trel

[1] Obtained from and identified by a Mexican herbalist.
[2] Obtained from Dr. J. W. Dollahite, Texas Agr. Expt. Sta., Marfa, Texas.
[3] Obtained by Mr. Jack L. Groff, County Agent in Bandera County.
[4] Obtained from S. B. Penick & Co., New York, N.Y.
The plant names are those used by Cory (1937) or Small (1913).

plete inhibition, by this method. Extracts from twelve plants inhibited the growth of well established tumor transplants when administered in five consecutive daily injections. The relatively non-toxic extracts of *Liatris punctata* inhibited tumor growth by all three methods.

The authors thank Dr. Benjamin C. Tharp, Professor of Botany, and Dr. C. C. Albers, Professor of Pharmacognosy, for the identification of many of these plants.

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THERMAL STRESS AND TISSUE CULTURE

VI. The Effect of Heat and Time of Exposure on Human Epidermis and Cells from the Skin and Leg Muscle of Chick Embryos*

J. Nakai,† C. M. Pomerat, and T. G. Blocker, Jr.

Tissue culture methods which have proven useful in the study of frostbite suggested an important challenge for a parallel analysis of burn injury. The first goal consisted of determining the amount of heat applied for various time intervals to several kinds of tissue explants which would be necessary to produce slight and moderate injury as well as total inhibition of outgrowth when the tissues were cultivated under standard conditions. On the basis of such information, a more searching inquiry might be made regarding the time course of events during heat injury with respect to the activity of various cell organoids such as mitochondria. Moreover, it was hoped that these data could serve as a basis for efforts directed at finding nutrients or drugs which might enhance recovery from heat injury (*cf.*, Pomerat, Nakai, and Blocker, 1954).

Materials and Methods

Heat injury was induced by placing thin-walled conical centrifuge tubes containing the test tissues suspended in 2 ml. of human ascitic fluid in a bath maintained at the desired temperature. The tubes were then returned to room temperature after a fixed time of exposure and the tissue fragments were placed in roller tube cultures for study according to the same procedure as that used for all other experiments in this series of reports.

More than 1,700 explants of adult human epidermis obtained in the course of split skin graft procedures and 2,700 explants of skin and leg muscle from 12- and 13-day chick embryos were used in this study. The plan of the experiments will be seen in the graphical summaries of the results(Figs. I–XII) and in tables 1 and 2.

Observations. The outgrowth resulting from 6 days' incubation of fragments exposed to heat for varying periods was recorded from preparations

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stained with Jacobson's method. For each temperature and time interval 20 fragments were generally used. In comparison with unheated controls, outgrowth in less than 50 per cent of the test cultures was rated as "marked injury" while series giving outgrowth in more than 50 per cent were characterized as showing "slight injury." The results have been summarized graphically in figures I–XII.

TABLE 1

Summary of Experiments on the Effect of Heat Exposure for Various Time Intervals to Adult Human Skin Fragments Subsequently Cultivated in Roller Tubes

Expt. Nos.	Number of Explants*	Time of exposure in minutes†		
		+45° C.	+47° C.	+49° C.
127–132	120			1‡, 1:25, 2:03, 5, 10:45
200–203	100			1, 1:10, 5:23, 15
204–207	100			3, 7, 12, 20
228–236	200	1, 5, 10, 15, 20, 25, 30, 35, 40		
262–265	100			1, 1:20, 3:20, 5
275–277 279	100			3, 4:46, 5:17, 7
282–285	100	15, 20, 40, 50		
538–543	140			4, 5, 6, 7, 8, 9
544–548	120	45, 60, 75,90, 120		
578–583	140	10, 40, 75, 100, 145, 170		
584–587	100			4, 5, 6, 8
588–599	260		1, 5, 10, 15, 20, 25, 30, 35, 40, 45, 50, 60	
600–608	200		3, 4, 6, 12, 18, 40, 60, 80, 100	

* Figures include unheated controls.
† Seconds are given following colon.
‡ Instantaneous.

A more detailed rating of outgrowth from human skin explants was performed with the use of a 10x ocular and a 16 mm. objective. Emigrating epithelial cells on all 4 sides of the fragments were measured in relation to the entire field and recorded as a weighed sum.

1. *Adult Human Skin.* Results obtained following exposure to heat at 45° C. are shown in figure I. Tissue fragments in tubes plunged into the

TABLE 2

Summary of Experiments on the Effect of Heat Exposure for Various Time Intervals to Chick Tissue Fragments Subsequently Cultivated in Roller Tubes

Expt. Nos.	Chick Age in Days	Number of Explants*	Time of exposure in minutes†		
			+45° C.	+47° C.	+49° C.
128–132	Sk. & Mus. 12	240			1‡, 1:25, 2:03, 5, 10:45
147–149	Sk. & Mus. 12	160			3:25, 6:17, 7
157–159	Sk. & Mus. 13	160	10, 30, 60		
428–436	Skin 12	200	1, 2, 4, 6, 8, 10, 30, 60, 120		
437–441 443	Muscle 12	140	5, 30, 40, 50, 60, 120		
454–460	Skin 13	160			1, 3, 5, 6, 8, 12, 15
462–467	Muscle 13	140			3, 5, 7, 9, 12, 15
627–633	Skin 13	160			3, 4, 5, 7, 9, 10, 15
634–638	Muscle 13	120			4, 6, 8, 10, 13
661–669	Skin 12	200	1, 0:30, 1:30, 20, 40, 50, 75, 90, 105		
670–677	Muscle 12	180	7, 9, 15, 20, 25, 55, 90, 150		
678–686	Skin 12	200		1, 2, 6, 7, 8, 10, 15, 20, 25	
678–686	Muscle 12	200		1, 4, 5, 12, 17, 22, 30, 65, 80	
702–707	Skin 13	140		25, 30, 35, 45, 55, 65	
708–711	Skin 13	80	110, 130, 150, 175		
712–717	Muscle 13	140		30, 35, 40, 45, 55, 60	
718–721	Muscle 13	80	110, 130, 160, 175		

* Figures include unheated controls.
† Seconds are given following colon.
‡ Instantaneous.

bath and quickly withdrawn (instantaneous) showed no subsequent evidence of injury but residence for as little as 5 minutes produced outgrowth in only 70 per cent (Expt. 229). Slight injury was observed following 20 minutes (Expts. 232, 283) while at 25 minutes only 27 per cent outgrowth was obtained (Expt. 233). Between 60 and 145 minutes' exposure at 45° C. some irregularities in results were encountered. Whereas heating for as much as 120 minutes (Expt. 548) showed 1 per cent outgrowth, no cell emigration was found in experiments 580 (75 minutes) and 581 (100 minutes). However, total injury at 45° C. seemed to result after approximately 120 to 145 minutes.

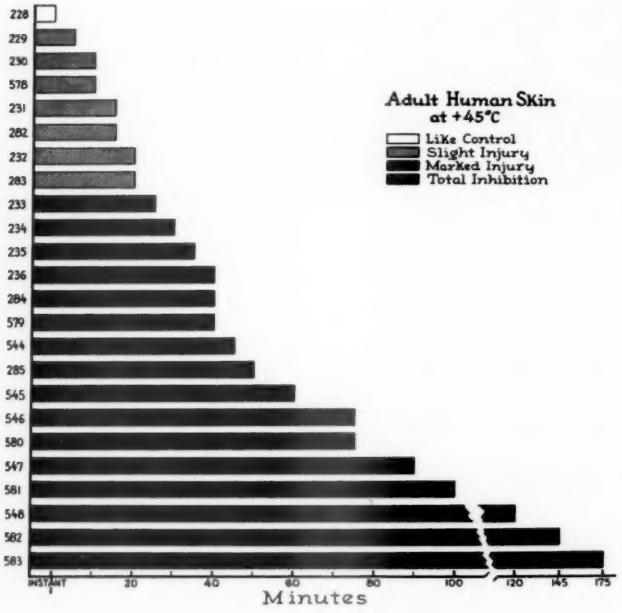

FIGURE I

Data for experiments conducted at 47° C. are given in figure II. The value for slight injury lays between (Expt. 588) and 3 minutes (Expt. 600). With exposure for 6 minutes (Expt. 602) 28 per cent of the cultures showed outgrowth but with 10 minutes (Expt. 590) only 16 per cent emigration was obtained. The point for total inhibition lay between 20 (Expt. 592) and 25 minutes (Expt. 593).

Injury reactions to heating at 49° C. are shown in figure III. Exposure for 2 minutes (Expt. 130) did not seem to affect the subsequent emigration

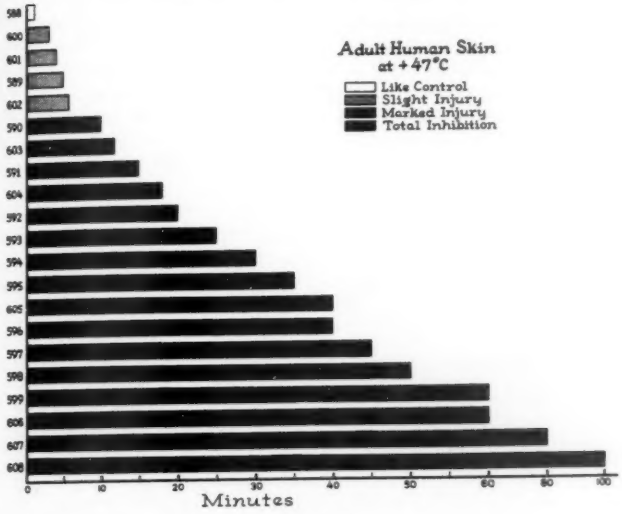

FIGURE II

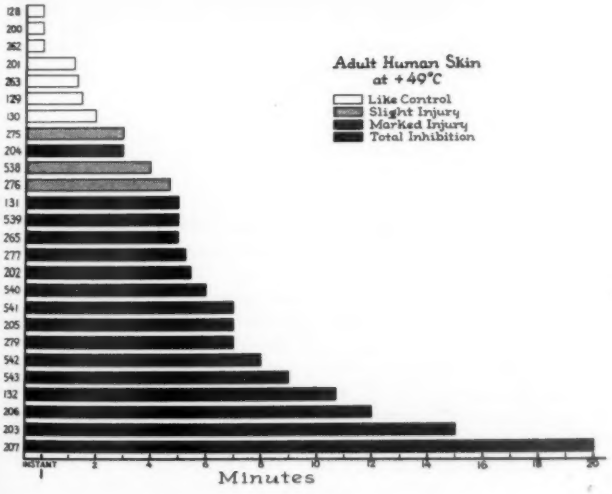

FIGURE III

of epithelial cells but damage was obtained with 3 minutes' exposure. In experiment 275, 60 per cent outgrowth was recorded while experiment 204 gave 2 per cent. The former was believed to be the more reliable value since 40 per cent was obtained with as much as 4¾ minutes (Expt. 276). After 5 minutes' contact (Expt. 131) outgrowth had been reduced to 21 per cent. Experiments 205 and 279 appear to be somewhat out of line in showing total inhibition with 7 minutes' exposure. After 8 minutes at 49° C., 4 per cent emigration was observed in experiment 542 but no cells migrated from explants heated for 9 or more minutes.

A composite graph (Fig. IV) has been made of the slight and marked injury as well as total inhibition of outgrowth from explants of human epidermis in relation to heating at 45°, 47°, and 49° C. for varying periods ranging from 0 to 140 minutes. A marked point of inflection is seen for the readings made at 47° C.

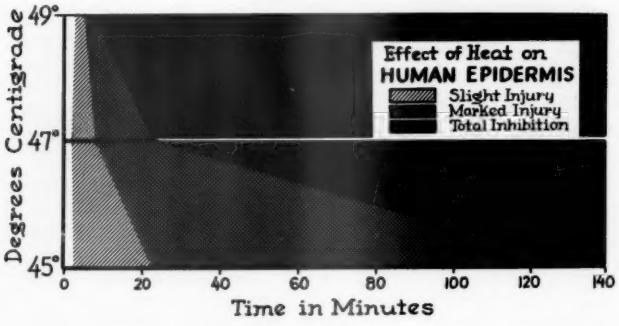

FIGURE IV

2. *Skin from 12- and 13-day Chick Embryos.* It was found convenient to represent the result obtained for the outgrowth of epidermal components (E) together with the corresponding spindle cells (S) from the dermal portion of the explants in each experiment.

Results obtained following heat at 45° C. are shown in figure V. Epithelial outgrowth was reduced even by instantaneous contact. Thus, in experiment 661, 67 per cent emigration was obtained while exposure for ½ minute (Expt. 662) gave 90 per cent and in experiment 663 heated for 1½ minutes outgrowth was reduced to 71 per cent. With 2 minutes' exposure marked injury was observed (49 per cent in Expt. 429). This reduction in epithelial cell emigration could be explained in terms of a relatively rich outgrowth of spindle cells as compared to the unheated control. Thus, even with very short exposure at 45° C. the connective tissue component was favored to the extent that the epidermal elements presented outgrowth distinctly inferior to that of the controls.

Results from experiment 157 appeared to be spurious but no explanation was found. In experiment 435 with 60 minutes' exposure epithelial out-

growth was reduced to 34 per cent while in experiment 159 under the same experimental conditions total inhibition was observed. However, it is believed that complete epidermal injury occurs between 60 and 75 minutes (Expt. 667).

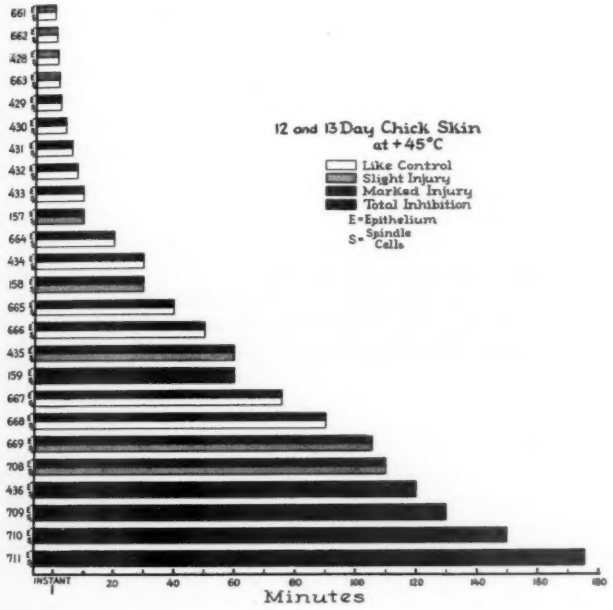

FIGURE V

Spindle cells are clearly more resistant to the unfavorable conditions imposed by heat. With 4 exceptions in 19 experiments, spindle cell outgrowth was equivalent to that of corresponding controls but following exposure for 105 minutes (Expt. 669) the outgrowth was reduced to 76 per cent. Marked inhibition fell between 110 (Expt. 708) and 130 minutes (Expt. 709). Total injury was produced in from 150 (Expt. 710) and 175 minutes (Expt. 711).

Results for heat injury to chick skin at 47° C. is shown in figure VI. With one exception (Expt. 679) the epithelium was unimpaired following heating for as much as 7 minutes (Expt. 681); however, with 8 minutes' exposure (Expt. 682) slight injury was observed. Marked injury occurred between 20 (Expt. 685) and 25 minutes (Expt. 686). With exposure for 35 minutes (Expt. 704) only 1 out of 18 cultures showed outgrowth while 45 minutes (Expt. 705) produced total inhibition.

Cultures not showing injury of spindle cells as compared to corresponding controls were found with exposure to 47° C. for as long as 25 minutes

(Expts. 686 and 702), while slight injury occurred with 30 minutes (Expt. 703), marked injury with 35 minutes (Expt. 704) and total inhibition with 45 minutes (Expt. 705).

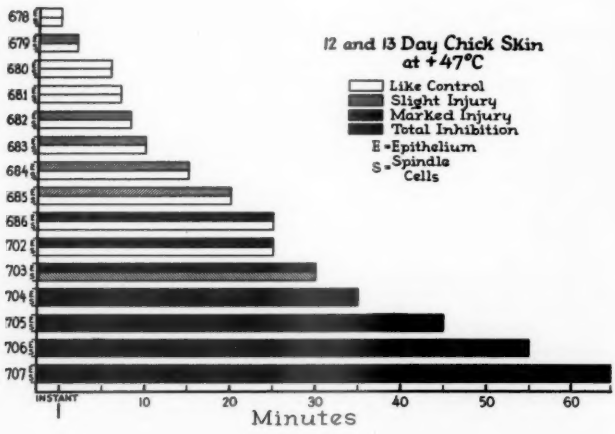

FIGURE VI

Results obtained at 49° C. for chick skin are shown in figure VII. The first evidence of injury was obtained between 1 (Expt. 454) and 1½ minutes (Expt. 129). With the exception of one experiment (147) slight injury was found for explants exposed from 1½ to 6 minutes (Expt. 457) in which 10 out of 18 cultures showed outgrowth. However, in another series under identical conditions (Expt. 148) 7 out of 14 had emigrating cells. This evidently was the critical point between slight and marked injury according to our rating. Experiments from 630 with an exposure of 7 minutes through experiment 632 which was subjected to heat contact for 10 minutes, all showed marked injury with one exception (Expt. 149). Total injury was produced between 10 (Expt. 632) and 10¾ minutes' contact at this temperature (Expt. 132).

With three exceptions (Expts. 455, 149, and 458) no reduction in spindle cells was obtained with as much as 10 minutes' exposure (Expt. 632) at 49° C., but beyond this point (10¾ min.) marked injury was observed and no spindle cells were found with heating for 15 minutes.

3. *Muscle from 12- and 13-day Chick Embryos.* For experiments performed at 45° C. (Fig. VIII) direct observation of stained preparations which had had short term exposure revealed a relatively rich pattern of outwandering spindle cells. It is believed that this may have created unfavorable conditions for the emigration of multinuclear fibers. For example, some evidence of damage to muscle was found following exposure for as little as 7 minutes (Expt. 670) although variation in results was found in experiments 671 and

157 (9 and 10 minutes). However, slight injury was recorded in from 15 (Expt. 672) to 55 minutes (Expt. 675). A careful study of the permanent slide preparations showed marked injury to muscle following 60 (Expt. 441) to 110 minutes (Expt. 718), while total inhibition appeared to have become manifest in this series with as little as 120 minutes (Expt. 442).

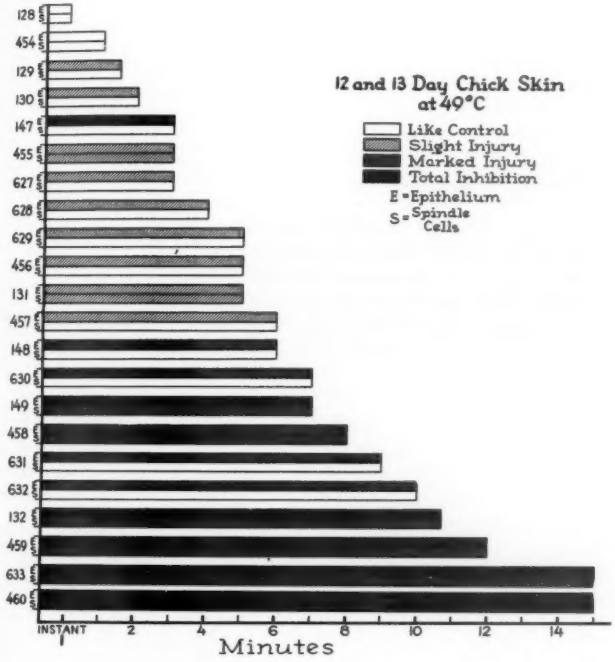

FIGURE VII

With the exception of one member (Expt. 159) of a pair of experiments heated to 45° C. for 60 minutes, spindle cells appeared uninjured. With as much as 90 minutes' exposure (Expt. 676), slight injury was seen with 110 (Expt. 718), marked injury with 120 (Expt. 442) and total injury with 175 minutes' exposure (Expt. 721).

Experiments performed at 47° C. (Fig. IX) were remarkable in that no evidence of injury to both muscle and spindles was observed with as much as 22 minutes (Expt. 683). However, muscle fibers were markedly injured at 30 minutes (Expts. 684 and 712) and totally inhibited following heating for 35 minutes (Expt. 713).

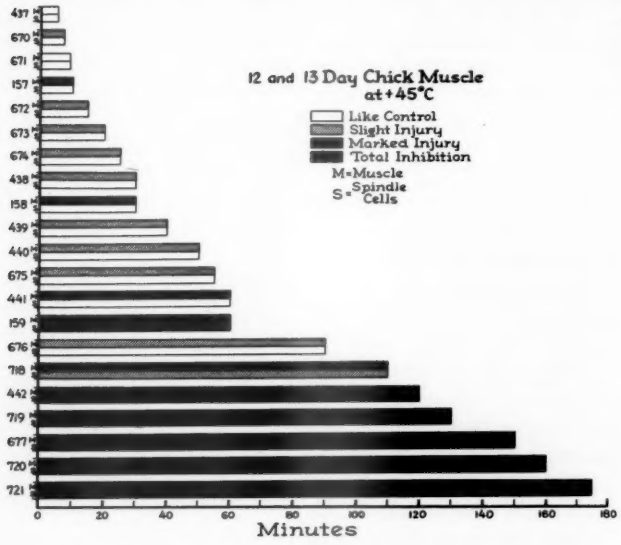

FIGURE VIII

Spindle cell injury at 47° C. began at 30 (Expt. 712) and became marked by 35 minutes (Expt. 713). Total inhibition was first seen following 45 minutes' exposure (Expt. 715).

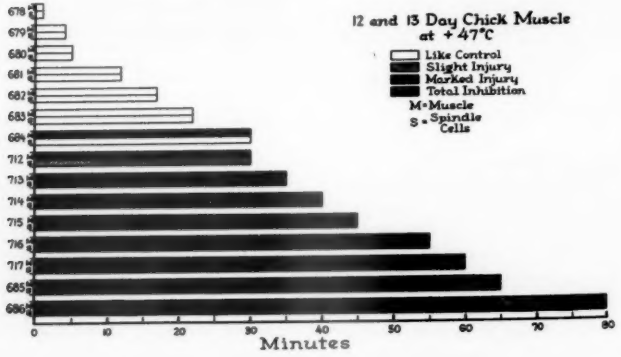

FIGURE IX

Figure X shows results for experiments in which chick muscle was heated at 49° C. In spite of an untoward effect at 5 minutes (Expt. 131) it is believed that this period marked the limit of non-injury to the muscle fibers (see Expt. 634). Slight injury was clearly indicated after 5 minutes (Expt. 148) while 6 minutes appeared to be the borderline between this rating and that of marked injury. After 10 minutes (Expt. 637) marked injury was recorded while at 10¾ (Expt. 132) and beyond muscle fibers were observed in the outgrowth.

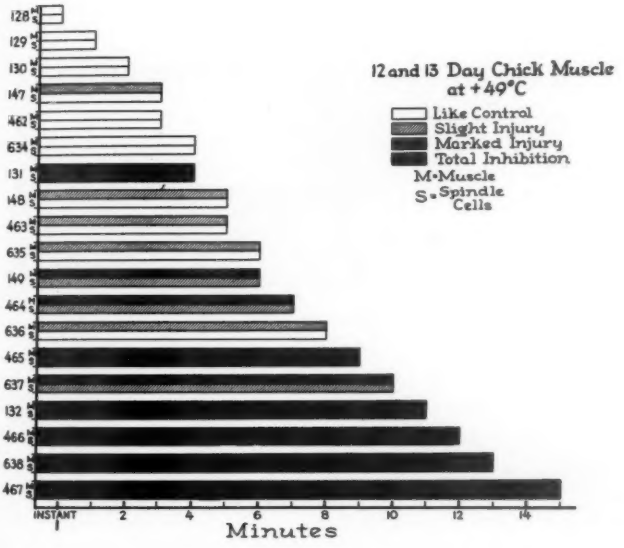

FIGURE X

Corresponding spindle cells exposed to 49° C. with one exception showed no injury after as long as 6 minutes (Expt. 635). Slight injury occurred between 6 and 10 minutes although the record showed some variation. Marked spindle cell injury was found at 10¾ minutes (Expt. 132) and total inhibition at 13 minutes (Expt. 638).

Composite graphs showing slight and marked injury as well as total inhibition of outgrowth after heating to 45°, 47°, and 49° C. for various periods are shown for chick skin in figure XI and for chick muscle in figure XII.

In contrast to the explants of human split skin graft material which produced an outgrowth consisting exclusively of epithelial cells, chick skin explants contained both epidermal and dermal elements. Cultures of both chick skin and leg muscle were consistent in revealing that the spindle elements were more resistant to heat injury than corresponding non-connec-

tive tissue cells. Moreover, the lines describing heat injury effects in figure XI for epithelial cells and for muscle fibers (Fig. XII) showed some peculiar differences in direction. At 45° C. the values for slight and marked injury to epithelial cells required considerably less time than for corresponding effects at 47° C. A similar observation was made for muscle at 45° C. These three unexpected results are not believed to have been due to heat, *per se*, but to the influence of competing spindle cells. It appeared that epidermal cells and muscle fibers grew less well than might be expected at 45° C. due to the heavy outgrowth of spindle cells. With increased heat injury to the spindle elements both epithelium and spindle fibers overcame this disadvantage.

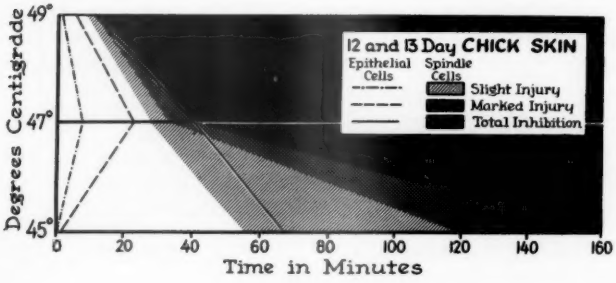

FIGURE XI

Discussion. The comprehensive list of references dealing with the effect of heat on cell systems studied with tissue culture technique given in Murray and Kopech's *A Bibliography of the Research in Tissue Culture* (1953) shows a wide variety of experimental attacks (*cf.*, Friedgood, 1928; Nemoto, 1929; Kokott, 1930, 1931; Vollmar, 1940). Heat was imposed upon primary explants or cell strains either before or after outgrowth had com-

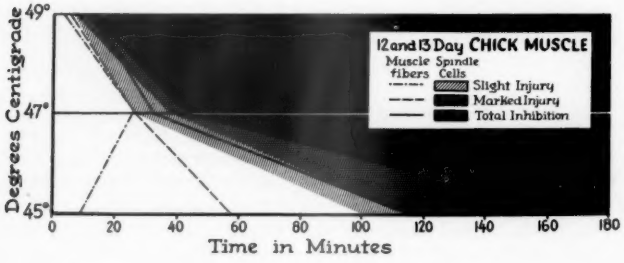

FIGURE XII

menced. Several reports are of special interest in relation to our findings.

Pincus and Fischer (1931) used sister cultures of a chick osteoblastic strain in Carrel flasks. One of the flasks was exposed to heat after the clot had set. No lethal effect was observed on subsequent culture after prolonged exposure to 42 and 44° C. but the cells were killed at 47° C. in 105 minutes, at 50° C. in 6 minutes, and at 52° C. in 3½ minutes. There appeared to be some retardation of outgrowth after exposure to all temperatures above 44° C. Mild injury was observed after a lag period and complete recovery occurred when the level of temperature and time of exposure were not too damaging. Some evidence was presented suggesting that the injury effects were not primarily due to interference with the nutrients in the clotted medium.

Kemp and Juul (1931) heated 48-hour cultures of chick embryo heart fragments and then observed them from 1 to 20 hours. Exposure to 50° C. for 20 minutes produced irreversible damage but recovery occurred following less than 5 minutes' heat contact. Injury from exposure within 1 hour to 47 and 48° C. was reversible.

Hetherington and Craig (1939) tested the survival of cells from 11-day chick embryos in several interesting types of experiments. Entire embryos, whole hearts and heart fragments were exposed to temperatures ranging from −70° C. to +52° C. and subsequently observed in hanging-drop cultures. No outgrowth was found after exposure to +50° C. for 30 minutes, while heating to 40, 45, and 47° C. for 30–40 minutes provided reversible injury. Cells survived heat stress less well when they were derived from whole embryos than from cut fragments. The best condition for survival appeared to result when heat was applied to fragments maintained in Ringer-Tyrode containing 0.2 per cent glucose. These observations led to the suggestion that the availability of 0_2 and of a ready energy source was important for the mechanism of tissue survival under deleterious conditions.

The design of our own experiments was based on the notion that it was preferable to avoid heating the constituents of the culture medium. Ascitic fluid was used as a desirable medium in which to expose the explants to the heat injury due to its resemblance to tissue juice.

As it had been observed in corresponding studies on the effect of cold, spindle-shaped elements were the most resistant components of the outgrowth. Little difference was found in the injury to the spindle cells from chick skin as compared to those emigrating from chick leg muscle.

Chick epithelium seemed slightly more vulnerable to heat than chick muscle fibers, while adult human epidermis was considerably more easily damaged at 49 and 47° C. than chick embryo skin, but at 45° C. the human cells were more resistant. It must be re-emphasized that the human explants were practically free of dermal constituents and rarely showed outwandering spindle cells. In contrast, dense feltworks of connective tissue elements were the rule in culture of chick embryonic skin. These differences constituted one of the most interesting features of the problem since stained preparations consistently provided evidence of differential competition between both epithelial cells and muscle fibers in relation to the corresponding stromal elements. It appears that with thermal injury the balance existing between these species of cells is shifted in relation to the available surface for outgrowth and contact with nutrients.

Summary

The effect of exposure to heat on the subsequent outgrowth of cells has been studied with the use of more than 4,400 tissue explants.

Fragments of adult human skin from split graft material as well as skin and muscle from 12- and 13-day chick embryos were heated to 45, 47, and 49° C. in a medium consisting of undiluted human ascitic fluid. The periods of exposure extended from instantaneous to 180 minutes.

Results are given in terms of slight and marked injury as well as for total inhibition. Human epidermis proved to be consistently more vulnerable to heat injury than chick embryonic skin. Multinuclear muscle fibers appeared to be more resistant than corresponding chick embryonic epithelial cells. As in the case of previous findings with the use of similar experimental procedures in the study of cold injury, spindle cells proved to be considerably more resistant to heat stress than both epithelial and muscle elements.

It is suggested that the values reported for graded and total injury to thermal stress provide reference information essential for further studies on the mechanism of cold and heat injury which can be studied profitably with tissue culture methods, especially with the technique of phase contrast, time lapse cinematography. Such records provide information regarding changes in cell dynamics as represented by shifts in the rate of membrane activity, mitochondrial movements and related phenomena. These data may prove valuable in the quest for nutritional components or drugs which may be useful in the recovery from thermal injury.

Acknowledgment. Grateful acknowledgment is made to Mr. McDonald Smith for his help in the preparation of the graphs.

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RESPONSE OF THE SYRIAN HAMSTER TO ELEVEN TROPICAL VIRUSES BY VARIOUS ROUTES OF EXPOSURE*

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The viruses employed in this study were the Zika and Ntaya viruses (Smithburn and Haddow, 1951), Wyeomyia virus (Garcia-Roca, 1946), Bwamba virus (Smithburn, Mahaffy, and Paul, 1941), Semliki Forest virus (Smithburn and Haddow, 1942), Bunyamwera virus (Smithburn, Haddow, and Mahaffy, 1946), Ilheus virus (Laemmert and Hughes, 1947), Uganda "S" virus (Rivers, 1948), Anopheles "A" and Anopheles "B" viruses (Garcia-Roca, 1946), and West Nile virus (Smithburn, Hughes, and Burke, 1940). All of the above viruses were furnished by the American Type Culture Collection in Washington, D. C. (Public Health Permit was obtained to work with these viruses). These viruses which were isolated in subtropical and tropical countries were obtained by the above institution from Dr. K. C. Smithburn of the Rockefeller Foundation Laboratories in New York. The object of this experiment was to determine the susceptibility of the Syrian hamster to these viruses by various routes of exposure.

Materials and Methods

Three-week-old Swiss albino mice (Webster strain) were employed. This strain was the one recommended by Webster (1935) as being especially susceptible to neurotropic viruses. The hamsters employed were 18 to 21 days old and healthy. They were purchased from a local dealer in Maryland.

Ten three-week-old Swiss albino mice were inoculated intracerebrally with 0.03 cc. of lyophilized mouse brain from each strain of the 37th mouse passage of the Bunyamwera strain, of the 204th mouse passage of the Wyeomyia strain, of the 20th mouse passage of the Uganda "S" strain, of the 26th mouse passage of the Ilheus strain, of the 165th mouse passage of the Anopheles "A" strain, of the 175th mouse passage of the Anopheles "B" strain, of the 82nd mouse passage of the Bwamba strain, of the 146th mouse passage of the Zika strain, of the 16th mouse passage of the Ntaya strain, of the 6th mouse passage of the Semliki Forest strain, and of the 26th mouse passage of the West Nile strain. After an incubation period of between four

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to eight days the mice from all groups showed nervous symptoms and mild tremors and paralysis. When symptoms appeared the sick mice from each group were sacrificed, and the brains were removed aseptically and kept in separate pools for each strain. Pools of each strain of infected mouse brain were ground with alundum and diluted to a 20 per cent suspension with physiological saline.

The eleven infected mouse brain pools were then contrifuged for five minutes in an angle centrifuge at 2,000 r.p.m. The supernatant fluids were removed from all eleven pools separately, and the sediments discarded. Each virus pool was injected intracerebrally into four unvaccinated and into four vaccinated mice. In six to ten days the unvaccinated mice from each pool showed nervous symptoms, while the immunized mice from each strain appeared normal thus confirming the identity of each virus used in this experiment.

Three hundred and fifty-two healthy Syrian hamsters (aged 18 to 21 days) were divided into 88 groups of 4 hamsters each. A 20 per cent suspension of each of the eleven viruses employed (Anopheles "A," Anopheles "B," Bunyawmera, Bwamba, Ilheus, Ntaya, Semliki Forest, Uganda "S," West Nile, Wyeomyia, and Zika) was inoculated into eight groups of four hamsters each in the following dosages and by the following routes: 0.03 cc. intracerebrally to the 1st group, 0.03 cc. intranasally to the 2nd group, 0.1 cc. intralingually to the 3rd group, 0.1 cc. intraorally to the 4th group, 0.1cc. intradermally to the 5th group, 0.1 cc. intrarectally to the 6th group, 0.03 cc. intracardially to the 7th group and 0.1 cc. intramuscularly to the 8th group. For the rectal instillation the end of a 22-gauge needle was filed off, rough ends smoothed down, and the tip lubricated with vaseline before being inserted.

Results

Hamsters from all groups except those inoculated with Anopheles "A" virus intraorally; Anopheles "B" virus intranasally, intralingually, intraorally, intradermally, intrarectally, and intramuscularly; Bwamba virus intracerebrally, intranasally, intralingually, intraorally, intradermally, intrarectally, and intramuscularly; Ilheus virus intraorally; Ntaya virus intralingually, intraorally, intradermally, and intrarectally; Semliki Forest virus intraorally; Wyeomyia virus intraorally; and Zika virus intranasally, intralingually, intraorally, intradermally, intrarectally, intracardially, and intramuscularly showed symptoms of nervous system involvement as shown in table 1. Separate suspensions were made from the brains removed aseptically from the hamsters showing symptoms of nervous system involvement and inoculated intracerebrally into four unvaccinated mice and into four mice immunized with each respective strain. In

four to ten days the unvaccinated mice from all strains stated showed nervous symptoms while the immunized mice from all strains stated appeared normal. It can be concluded that hamsters inoculated with the various virus strains stated which showed any symptoms of central nervous system involvement were susceptible to the respective virus by the routes stated in table 1. The surviving hamsters were held 21 days and showed no symptoms of central nervous system involvement.

TABLE 1

Response of Hamsters to Eleven Tropical Viruses Showing Number of Hamsters in Each Group Which Developed Symptoms

Virus	Intracerebrally	Intranasally	Intralingually	Intraorally	Intradermally	Intrarectally	Intracardially	Intramuscularly
Anopheles "A"	4	4	4	.	4	4	4	4
Anopheles "B"	4	.	.	.	.	.	4	.
Bunyamwera	4	4	4	4	4	4	4	4
Bwamba	.	.	.	.	.	.	4	.
Ilheus	4	4	4	.	4	4	4	4
Ntaya	4	4	.	.	.	.	4	4
Semliki Forest	4	4	4	.	4	4	4	4
Uganda "S"	4	4	4	4	4	4	4	4
West Nile	4	4	4	4	4	4	4	4
Wyeomyia	4	4	4	.	4	4	4	4
Zika	4	.	.	.	.	.	.	.

Minimum to maximum incubation period (time elapsing between inoculation and onset of symptoms) in all cases was 5 to 7 days.
Four hamsters were exposed to each virus by each route of inoculation.

Summary. It was found in this experiment that the Syrian hamster is susceptible to the Anopheles "A" virus intracerebrally, intranasally, intralingually, intradermally, intrarectally, intracardially, and intramuscularly; to the Anopheles "B" virus intracerebrally and intracardially; to the Bunyamwera virus intracerebrally, intranasally, intralingually, intraorally, intradermally, intrarectally, intracardially, and intramuscularly; to the Bwamba virus intracardially; to the Ilheus virus intracerebrally, intranasally, intralingually, intradermally, intrarectally, intracardially, and intramuscularly; to the Ntaya virus intracerebrally, intranasally, intracardially, and intramuscularly; to the Semliki Forest virus intracerebrally, intranasally, intralin-

gually, intradermally, intrarectally, intracardially, and intramuscularly; to the Uganda "S" virus intracerebrally, intranasally, intralingually, intraorally, intradermally, intrarectally, intracardially, and intramuscularly; to the West Nile virus intracerebrally, intranasally, intralingually, intraorally, intradermally, intrarectally, intracardially, and intramuscularly; to the Wyeomyia virus intracerebrally, intranasally, intralingually, intradermally, intrarectally, intracardially, and intramuscularly; and to the Zika virus intracerebrally.

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THALAMO-CORTICAL RELATIONSHIPS IN MAN*

I. A Report of a Case of Old Cortical Softening of the Parietal and Frontal Lobes with Atrophy of the Right Dorsal Thalamus

Glenn V. Russell

Advances in our knowledge and understanding of thalamo-cortical relationships in man have been based principally upon investigations on the cat and the monkey. In only about 100 human cases have thalamic changes following cortical lesions been studied, and the majority of these have been concerned with prefrontal lesions only. It would seem appropriate, therefore, to evaluate alterations of the cytoarchitecture of the nuclei of the dorsal thalamus following cortical and/or subcortical lesions in a selected series of human cases in order to further elucidate the projections of these nuclei. The cortical area or areas to which some of the thalamic nuclei project seem firmly established but some problems have remained unresolved. These include: (1) the possible cortical projection of the nucleus reticularis thalami; (2) the possible cortical projection of the nucleus ventralis anterior; and (3) the possible direct cortical projection of the intralaminar nuclei, in particular the nucleus centrum medianum and nucleus paracentralis. Efforts will be made, in the report of this case and of the cases to follow, to shed light upon these problems.

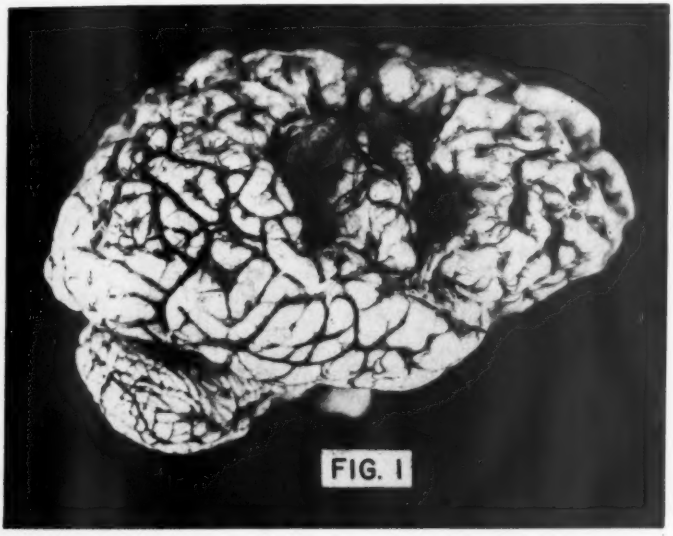
Case Report

Clinical History. The brain is that of a 70-year-old white male (T. L.) who was brought to the Emergency Room of John Sealy Hospital on March 1, 1954, in shock and with no immediately available history. The past history that could be obtained indicates that the patient had a sudden "weak spell" in May, 1947, at which time he fell and became paralyzed on the entire left side of his body. At no time was a sensory disturbance noted. In August, 1947, there had been marked recovery of function of the left side of his face and some recovery of function in his left arm and leg. In December, 1947, he developed episodes of generalized convulsions lasting two to five minutes. At that time the chart notes that a moderate hemiplegia was evident on the left side of his body. In 1953 the patient was

* From the Department of Anatomy, The University of Texas Medical Branch, Galveston. Received for publication June 1, 1954.

treated as an outpatient in the G. U. Clinic of the John Sealy Hospital for a draining suprapubic sinus. The hemiplegia was still present. His terminal admission was on March 1, 1954, at 1:00 P.M. He died at 1:40 P.M. without regaining consciousness. The cause of death was established as a myocardial infarction complicated by generalized infection and bronchopneumonia.

Necropsy. Post-morten examination of the brain revealed four prominent areas of old cortical softening in the right hemisphere (Fig. 1). The largest



Right lateral view of the brain of T. L: The island of cortex visible between the areas of encephalomalacia is identifiable as the pre- and post-central gyri.

of these areas was in the right frontal region and involved a large part of the right middle frontal and superior frontal gyri where these were supplied by branches of the middle cerebral artery. The second area of old softening was connected to the first by a narrow isthmus and had destroyed the pars opercularis and pars triangularis of the right inferior frontal gyrus. The latter lesion had impinged upon the lower part of the precentral gyrus, although this gyrus above the level of the inferior frontal sulcus was not apparently involved. The third lesion had destroyed most of the superior surface of the right temporal lobe, the opercular portion of the right parietal lobe, the anterior one-half of the supramarginal gyrus, and the posterior gyri of the insula. The last area of old softening involved cortex on the medial surface of the right hemisphere and had destroyed the middle portion of the gyrus cinguli and the posterior part of the superior frontal gyrus in the

region of cortical areas (after Brodmann) anterior 23 and 31 and posterior 24. This latter area of encephalomalacia is in the region of the distribution of the right anterior cerebral artery while the first three lesions were in the distribution of the pre-Rolandic and posterior parietal (supramarginal) branches of the right middle cerebral artery. It is to be noted that the right precentral (except for its inferior 1 cm.) and postcentral gyri were spared, suggesting that the Rolandic branches of the right middle cerebral artery were not involved.

The left cerebral hemisphere showed no particular pathology and the brain stem and the cerebellum were not grossly remarkable. The vessels at the base of the brain were small and showed only scattered atheromatous plaques.

Coronal sections through the cerebral hemispheres revealed the cortical softenings described above and, in addition, almost complete destruction of the head of the caudate nucleus and a large part of the anterior limb of the internal capsule on the right side. A part of the anterior portion of the right putamen where it lies adjacent to the anterior limb of the internal capsule was also involved by this lesion. The right dorsal thalamus showed marked atrophy. The internal capsule and the basis pedunculi on the right side appeared reduced in size. Gross sections through the brain stem and the cerebellum were not remarkable other than for a slight reduction in the size of the right pyramid.

The diencephalon was trimmed, divided into four blocks, imbedded in celloidin, and serially sectioned in the frontal plane. Selected sections were stained for cells with buffered thionin and for fibers by the method of Smith and Quigley, and mounted for microscopic examination. The author is indebted to Dr. K. M. Earle, Director of the Laboratory of Neuropathology of the Medical Branch, for making available the gross material; and to Mrs. Jeanne Batts for technical assistance in the preparation of the sections.

Microscopic Examination. No microscopic examination of the cerebral cortex was made; however, careful study of the brain suggests that the following cortical areas had been destroyed on the lateral convexity of the right cerebral hemisphere: cortical areas (after Brodmann) 6, 8, 41, 42, 44, 45, the inferior 1 cm. of 4, the anterior one-half of 40, and area 52 and J. posterior. These lesions may have undercut more of area 4 (based on the residual hemiplegia) but this was not evident. Sections through the medulla show the right pyramid to be moderately reduced in size, but with many normal fibers still present. On the medial surface of the right cerebral hemisphere the following cortical areas were destroyed: cortical area 6, the anterior portions of 23 and 31, the posterior portion of 24, and probably a part of 4.

Sections through the rostral levels of the basal ganglia reveal that a large portion of the head of the caudate nucleus and the adjacent portions of the putamen, as well as the intervening anterior limb of the internal capsule, had been destroyed on the right side by an additional area of old softening. The body of the caudate nucleus, the globus pallidus, and the putamen adjacent to the latter nucleus were uninvolved.

Study of the serial sections taken through the diencephalon reveal a great reduction in the size of the right dorsal thalamus with evident shrinkage and/or absence of certain nuclei. The nucleus anterior on the right side is reduced to about one-half the size of the nucleus anterior on the left side (Figs. 2–3) and there is a marked cell-loss in the affected nucleus. The slight increase in glial nuclei in the affected nucleus is more compatible with glial concentration accompanying shrinkage in the size of the nucleus than with true gliosis. No neurons identifiable as belonging to the nucleus anterodorsalis are evident on the affected side and it is presumed that this nucleus has completely degenerated.

The nucleus medialis dorsalis on the right (Figs. 2–5) is reduced in size and throughout its antero-posterior extent only a few scattered neurons remain intact. The majority of these remaining neurons are of fairly large size and are characteristic of the pars magnocellularis of this nucleus. There is no evidence of any true nucleus medialis dorsalis pars parvocellularis on the affected side. Glial condensation rather than true gliosis is again apparent, there being only about twice the number of glial nuclei in the atrophic nucleus as in the normal nucleus on the left side.

Distortion in the midline region of the diencephalon in this case makes it difficult to establish the precise limits of the nuclei of the midline: nucleus parataenialis, nucleus paraventricularis, nucleus rhomboideus, and nucleus reuniens. It is apparent, however, that none of these nuclei are absent on the affected side.

The nucleus centrum medianum is the most affected of the nuclei of the intralaminar group in the right thalamus (Figs. 4–5). There is the suggestion of a slight increase in the cell-density of both neural and glial elements but this concentration is not nearly sufficient to account for the marked reduction of the size of the nucleus. At the rostral end of the nucleus centrum medianum the size reduction is accompanied by a severe cell-loss and a moderate gliosis. The nucleus parafascicularis (Fig. 5) is unaffected on the right side. Shrinkage of nuclei and glial changes adjacent to the internal medullary lamina make it difficult to assess possible alteration of the remaining nuclei of this group. A few clumps of scattered neurons identifiable by appearance and position as belonging to the nucleus paracentralis, the nucleus centralis lateralis, and the nucleus centralis medialis are seen. The scattered arrangement of the neurons of these nuclei in the normal thalamus must be mentioned. Nonetheless, on the affected side these nuclei are not prominent nor are they condensed as one would expct if all their neurons were present.

In the normal left thalamus (Fig. 3) the nucleus submedius may be identified; however, on the affected side the rather dense glial accumulation in the ventral region of the internal medullary lamina rostral to the nucleus centrum medianum precludes the absolute identification of this nucleus.

Examination of the thalamic nuclei which lie external to the internal medullary lamina, the ventral and lateral nuclear complexes, reveals that it is the ventral group which is more severely affected. The right nucleus ventralis anterior is almost completely degenerated and the region it occupied

is reduced to a condensation of glial nuclei (Fig. 2). A few scattered neurons remain in the dorsomedial area of the nucleus but no neurons are visible in

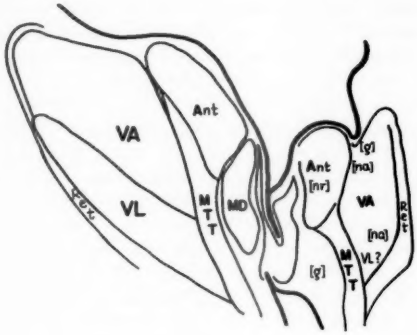


FIG. 2

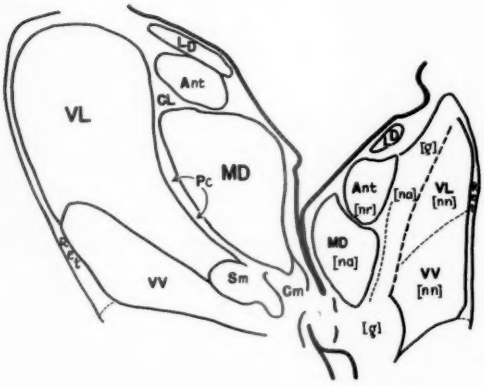


FIG. 3

FIGS. 2–5. Thalamic nuclei at four levels in the case of T. L. Atrophy of nuclei of the right dorsal thalamus is evident.

Abbreviations for all figures: Ant, is the nucleus anterior; Cl, the nucleus centralis lateralis; CM, the nucleus centrum medianum; Cm, the nucleus centralis medialis; [g], gliosis or glial concentration; Hb, the habenular nuclei; HPT, the habenulopeduncular tract; LD, the nucleus lateralis dorsalis; LG, the corpus geniculatum laterale; LP, the nucleus lateralis posterior; MD, the nucleus medialis dorsalis; MG, the corpus geniculatum mediale; MTT, the mammillo-thalamic tract; [na], neurons absent or severely reduced; [nn], neurons virtually normal; [nr] moderate cell-loss; Pc, the nucleus paracentralis; Pf, the nucleus parafascicularis; Pul, the pulvinar; Ret, the nucleus reticularis thalami; Sm, the nucleus submedius; VA, the nucleus ventralis anterior; VL, the nucleus ventralis lateralis; VPi, the nucleus ventralis posteroinferioris; VPl, the nucleus ventralis posterolateralis; VPm, the nucleus ventralis posteromedialis; and VV, the nucleus ventralis ventralis.

the ventrolateral portion. The nucleus ventralis lateralis (Fig. 3) is smaller on the affected side. The neurons which remain in this nucleus are arranged in their normal density ventrolaterally, but are greatly reduced in number in the dorsomedial portion (Fig. 4). The ventramedial portion of the nucleus ventralis lateralis (nucleus ventralis ventralis) appears unaffected.

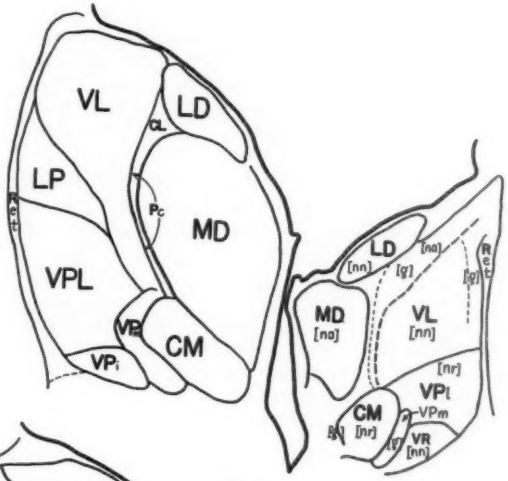


FIG. 4

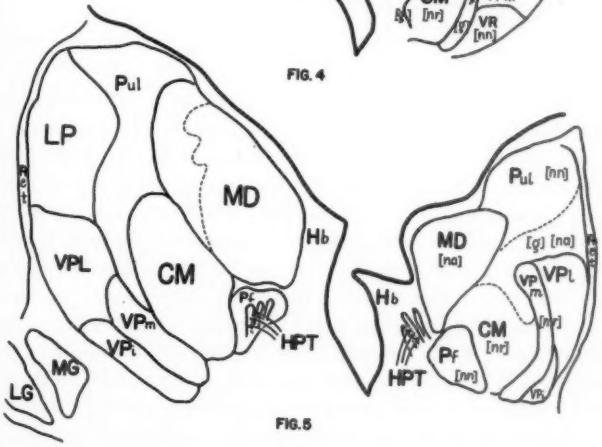


FIG. 5

The ventral posterior nuclear complex (Figs. 4–5) demonstrates atrophic changes which had not been anticipated. The nucleus ventralis posterolateralis is reduced in size and appears to have undergone a moderate cell-loss. The nucleus ventralis posteromedialis (arcuatus) contains a moderate

glial condensation in its medial portion. In more lateral portions of this nucleus some slight size reduction is apparent. The nucleus ventralis posteroinferioris does not appear to have been affected. About the middle of the rostrocaudal extent of the nucleus ventralis posterolateralis there is a large glial accumulation almost devoid of neurons. This area may be a remnant of either the nucleus ventralis posterolateralis or of the nucleus lateralis posterior or of both.

The nuclei of the lateral nuclear complex (Figs. 4–5) are less affected. The nucleus lateralis dorsalis is not altered. The right nucleus lateralis posterior is reduced in size with a moderate cell-loss. The pulvinar is less affected. Only in the region of the junction between the latter two nuclei is there an indication of severe atrophy (Fig. 5). The more caudal regions of the pulvinar are unaltered.

The corpus geniculatus laterale shows no cellular changes in the affected thalamus. The corpus geniculatus mediale, however, has been reduced on the affected side to a glial scar with only a few scattered neurons remaining at the site of the nucleus.

The nucleus limitans is not altered on the affected side. The nucleus suprageniculatus has undergone some cell-loss. The nucleus reticularis thalami shows loss of neurons, particularly in its dorsolateral portions, although at no point is the nucleus completely devoid of apparently normal neurons. As one proceeds caudally through the nucleus from the fairly cellular anterior cap-like region there is first some atrophy and then a return towards the normal cytoarchitecture.

Comments

No final analysis of the cortical projection of any thalamic nucleus in man can be based upon the report of a single case, nor does a review of the literature indicate that any final determinations may be made for many of these nuclei. Comments, however, on the present observations in comparison with those previously published are desirable. The author agrees with the statement of Krieg (1948), concurred in by McLardy (1950), that the nucleus anteroventralis and nucleus anteromedialis are not distinguishable from each other in man on the basis of cytoarchitecture. In the present case it is further shown to be impossible to demonstrate such subdivisions of the nucleus anterior based upon a selective loss of cells in either the dorsolateral or the ventromedial portion of this nucleus. Rather the cell-loss and glial concentration is uniform throughout the affected nucleus. Although it has been reported by Powell (1952) that no cells remain in the nucleus anterior after hemidecortication in man, the precise cortical areas to which it projects have not been clearly

established. The presumptive area of the nucleus anteromedialis has been reported to project to area 24 in the rostral gyrus cinguli (Clark and D. S. Russell, 1940; McLardy, 1950). The presumptive area of nucleus anteroventralis has been reported to project to area 24 (Freeman and Watts, 1947), or perhaps areas 11 and 12 on the medial aspect of the frontal lobe (Kirschbaum and von Bonin, 1947). More recently it has been suggested that this presumptive area projects to more caudal portions of the gyrus cinguli (Clark and D. S. Russell, 1940), perhaps area 23 (McLardy, 1950). Kirschbaum and von Bonin (1947) were aware that such a projection might exist, but believed that it had to be rather small. The present case, with the rather marked reduction in the size and number of neurons in the nucleus anterior and the sparing of the anterior portion of the gyrus cinguli, supports the existence of a moderately large projection from the nucleus anterior to more caudal area of the gyrus cinguli, posterior area 24 and areas 23 and 31(?).

Powell (1952) failed to mention the nucleus anterodorsalis in his description of the degeneration of the thalamus in a hemidecorticate man, although it has been generally presumed that this nucleus projected to the cortex of the retrosplenial gyrus cinguli. The absence of this nucleus in the affected thalamus in the present case, where the lesion in the middle of the gyrus cinguli would have destroyed the projection from this nucleus to the retrosplenial cortex, is in agreement with this suggestion.

Absence of the pars parvocellularis of the nucleus medialis dorsalis in the present case must be attributed to the destruction of the anterior limb of the internal capsule rather than to any cortical lesion. The retention in the affected thalamus of the nucleus medialis dorsalis pars magnocellularis, represented by the rostral and medial lamina of larger neurons together with the scattered larger neurons in the central region of this nucleus, suggests that these neurons do not project to the cerebral cortex, at least not via the anterior limb of the internal capsule. These observations are in agreement with the reports of Norman (1945), Freeman and Watts (1947), McLardy (1950), and Powell (1952).

The connections, both afferent and efferent, of the nuclei of the internal medullary lamina remain one of the major unsettled problems of thalamo-cortical morphology. In the present case

a marked reduction of the nucleus centrum medianum is associated with combined destruction of the cortex and of the head of the caudate nucleus and adjacent putamen. That the degeneration of the nucleus centrum medianum must be attributed to the destruction of the latter regions of the basal ganglia would be in accord with the report of the Vogts (1941), Kirschbaum and von Bonin (1947), and Freeman and Watts (1947). The latter investigators reported complete degeneration of the nucleus centrum medianum as a result of a lesion destroying essentially all of the caudate nucleus and the putamen, but sparing the globus pallidus. In the present case the more caudal portions of the body and the tail of the caudate nucleus and the bulk of the putamen, as well as all of the globus pallidus, are spared and the nucleus centrum medianum is still a prominent nucleus, although markedly reduced. On the other hand, Powell (1952) has reported a marked reduction in the nucleus centrum medianum associated with such a small lesion in the caudate nucleus that he concluded that some other projection of this nucleus must exist. Norman (1945) and Freeman and Watts (1947) held that the projection of nucleus centrum medianum was entirely subcortical, but reports of cortical projections for this nucleus have appeared in the literature. Clark and W. R. Russell (1939) suggested that it projected to the insular cortex in man, but in a later publication (Clark and D. S. Russell, 1940) this was denied. McLardy (1950) reported the disappearance of this nucleus on the affected side with purely cortical lesions wherein the basal ganglia were not directly involved.

The nucleus parafascicularis in the present case was not affected, and it has been generally agreed that this nucleus is not involved after cortical lesions and that it does not, therefore, project to the cortex (Clark and W. R. Russell, 1939; Powell, 1952). The status of the remaining intralaminar nuclei; that is, the nucleus paracentralis, nucleus centralis medialis, and nucleus centralis lateralis; is not settled. The impression in the present case is that these nuclei are markedly affected and partly absent on the affected side. Degeneration of these nuclei has been reportd by Clark and D. S. Russell (1940) where there was almost complete cortical loss, and by Kirschbaum and von Bonin (1947) where the frontal cortex back to the level of the genu of the corpus callosum and the temporal tips was lost. Powell

(1952), on the other hand, reported gliosis but no cellular changes after essentially complete hemidecortication.

It is not clear to which nuclear group the nucleus submedius belongs. McLardy (1950), confirming the report of Krieg (1948) that this nucleus is inconsistent in man, has reported that it is present in only one-third of the 65 brains that he examined. When it was present, he reported, the nucleus submedius is located ventral to the nucleus medialis dorsalis and projected to cortical area 8. Freeman and Watts (1947) have reported that it projected to area 46. Since the nucleus submedius cannot be identified with assurance on the affected side in the present case it is not possible to support or refute these statements. The author is of the opinion, however, that the nucleus submedius belongs to the nuclei of the internal medullary lamina rather than being a part of the nucleus medialis dorsalis as implied by the reports of Freeman and Watts (1947) and McLardy (1950). Its position in the ventral aspect of the internal medullary lamina rostral to the nucleus centrum medianum, the wide scattering of its neurons, and the sparsity of myelinated fibers within it, suggests that the poorly myelinated and the unmyelinated fibers of the intrathalamic bundle must course rostrally through this nucleus, although there is no doubt that fibers from the nucleus medialis dorsalis which pass into the inferior thalamic peduncle must also course through this region.

The subdivision of the anterior portion of the ventral nuclear complex in man into a nucleus ventralis anterior and a nucleus ventralis lateralis is not agreed to by all authors. Toncray and Krieg (1946) were unable to differentiate these two nuclei on the basis of cytoarchitecture. Dekaban (1953) makes the distinction on both a cytoarchitectonic and on a functional basis. The observations in the present case of the almost complete degeneration of the region described as nucleus ventralis anterior with partial sparing of the region described as nucleus ventralis lateralis supports the latter view and agrees with the reports of Clark and D. S. Russell (1940) and of McLardy (1950). When viewed in the light of previous reports (Norman, 1945; Kirschbaum and von Bonin, 1947; Freeman and Watts, 1947; Meyer, 1949) it would seem justifiable to conclude that the degeneration of the nucleus ventralis anterior is directly related to the destruction of the premotor cortex (areas 6 and 8?). The mild

involvement in the present case of area 4 in the precentral gyrus suggests that this nucleus does not have more than a very small projection to that region. Earlier suggestions that this nucleus projected to both cortex and basal ganglia are not proven or disproven by the present case, but from earlier reports basal ganglia damage does not appear necessary for atrophy of the nucleus ventralis anterior. Powell (1952) has reported sparing of one-half of the neurons in this nucleus, and in the other nuclei of the ventral complex, 24 days after essentially complete hemidecortication. At present these reports have no satisfactory explanation.

In the present case the nucleus ventralis lateralis is described as moderately affected in its ventrolateral portion, but rather markedly affected in its dorsomedial portion. Stern (1942) had described the dorsomedial region as severely affected with lesions of cortical areas 9, 10, 45, and 46. Kirschbaum and von Bonin (1947) reported slight involvement of the medial portion of this nucleus, with some destruction of area 6 and extensive loss rostral thereto. McLardy (1950) concluded that the dorsomedial portion of this nucleus projects to more dorsal parts of area 6 and that the ventrolateral portion of the nucleus projects to ventral parts of area 6. He denied any projection from this nucleus to area 44 in man. From the present case it is suggested that the dorsomedial portions of the nucleus ventralis lateralis project to the premotor cortex of area 6 (and possibly 8), while the ventrolateral portions of the nucleus project to the motor area proper (area 4) and perhaps rostrally into ventral area 6, area 44, and area 45. These suggestions would conform to the generally accepted projections of this nucleus in primates (Walker, 1938). Again it must be stated that the apparent sparing of one-half the neurons of this nucleus 24 days after virtual hemidecortication as reported by Powell (1952) remains with no adequate explanation, although Powell has discussed it at some length.

As has been stated, the partial degeneration of the nuclei of the ventral posterior complex was not expected in the present case, the postcentral gyrus and underlying white matter having appeared to be intact and no sensory disturbances ever noted. It is therefore suggested that the nucleus ventralis posterolateralis and nucleus ventralis posteromedialis must have in addition to the generally accepted projection to the postcentral gyrus a

moderate projection to the adjacent and caudally lying parietal cortex. This area of cortex includes the portion of the parietal operculum and superior bank of the Sylvian fissure in which a secondary somesthetic area is described (Penfield and Rasmussen, 1950).

The nucleus lateralis dorsalis is unaffected in the present case. This nucleus does degenerate after total hemidecortication in man (Powell, 1952) but there is no evidence bearing on the specific cortical area to which it projects. The nucleus lateralis posterior and the pulvinar are likewise known to undergo complete degeneration following extensive parieto-occipito-temporal cortical ablations (Papez, 1939; Powell, 1952). Papez (1939) has reported that these projections in large part pass through the retrosplenial portion of the internal capsule in association with the visual radiations. Since the latter are not involved in the present case (complete sparing of the corpus geniculatum laterale is described), it is inferred that the destruction of the region of junction between these two nuclei and the nucleus ventralis posterolateralis must project to the parietal opercular and anterior supramarginal cortex (area 40).

No change is observed in the corpus geniculatum laterale on the affected side nor was any alteration expected since the striate cortex (area 17), which is the sole area of cortical projection for this nucleus (Clark and D. S. Russell, 1940), and the visual radiations were not involved by the lesion. The almost complete atrophy of the corpus geniculatum mediale was entirely expected, however, since the auditory area in the affected hemisphere (areas 41 and 42) was entirely ablated. That a few scattered neurons remain in this nucleus accompanied by a rather severe gliosis is in agreement with the report of Powell (1952).

The nucleus limitans is not involved in the present case. The failure of this nucleus to atrophy after cortical lesions has been reported (Papez, 1939; Powell, 1952) and it is generally agreed that it has no cortical connections. The status of the nucleus suprageniculatus is not as clear. The author has suggested that this nucleus is actually a part of the pulvinar, basing this suggestion on cytoarchitectonic similarities and on a report that it was degenerated in a case where the pulvinar was also markedly atrophic (Papez, 1939). Generally it has been believed that this nucleus does not have cortical connections; however, in the

present case a moderate cell-loss is evident in the affected nucleus. Powell (1952) failed to mention this nucleus, and it is felt that more data will be required to clarify the proper relationship and projection, if such exists, of this nucleus.

The nucleus reticularis thalami is, from a developmental and morphological standpoint, a portion of the ventral or subthalamus although it is generally described as a part of the dorsal thalamus. From a functional standpoint its close relationship to the latter region cannot be disputed. It has been inferred from studies on primate material that this nucleus undergoes degeneration in direct proportion to the degree of degeneration in the immediately adjacent portion of the ventral and/or lateral nuclear complex following cortical ablations. In man, Sheps (1945) and Freeman and Watts (1947) reported no observable changes in this nucleus after frontal lobe lesions, although both authors reported prominent changes in the nucleus ventralis anterior. Clark and D. S. Russell (1940) were impressed by the almost complete degeneration of this nucleus in a case with almost complete hemidecortication in which the entire ventral and lateral nuclear complex was also degenerated. McLardy (1950) received the impression that this nucleus was not as severely damaged as the adjacent ventral nuclei in his series of cases, and Powell (1952) although reporting marked cell-loss in this nucleus was unable to state whether there was a correlation between degeneration of the nucleus reticularis and the adjacent ventral and lateral nuclei. In the present case the most severe damage of the nucleus reticularis does appear to be more dorsal as is the more severe damage in the ventral nuclei, but the rostral pole of the nucleus lying adjacent to the almost completely atrophic nucleus ventralis anterior gives the impression of being not as markedly degenerated as some more caudal portions of the nucleus in the region of the less affected nucleus ventralis lateralis. Thus the present case would tend to support the impression of McLardy (1950).

Summary. A case with old cortical softening in the frontal, parietal, and temporal lobes has been reported and the atrophic changes in the nuclei of the dorsal thalamus described and commented upon. The following cortical projections are supported by this case: (1) a rather significant projection from nucleus anterior to more caudal portions of the gyrus cinguli (posterior

area 24 and areas 23 and 31?); (2) projection of nucleus anterodorsalis to the retrosplenial cortex; (3) a diffuse cortical projection from nucleus centrum medianum and the other intralaminar nuclei, excluding nucleus parafascicularis; (4) projection of nucleus ventralis anterior to the premotor cortex (areas 6 and 8?); (5) projection of dorsomedial nucleus ventralis lateralis to area 6; (6) projection of ventrolateral nucleus ventralis lateralis largely to area 4 but perhaps also to more rostral regions along the inferior frontal gyrus; (7) projection of the ventral posterior nuclear complex, in part, to the parietal operculum; (8) exclusive projection of the corpus geniculatum mediale to the cortex of the superior temporal gyrus and the inferior bank; and (9) a cortical projection from nucleus suprageniculatus. No well-defined correlation between the degeneration of the nucleus reticularis thalami and the adjacent ventral and/or lateral nuclei was observed. The inclusion of nucleus submedius with the nuclei of the internal medullary lamina was suggested.

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PAPER CHROMATOGRAPHIC INVESTIGATION OF CHOLESTEROL-C^{14} AND RADIOACTIVE CONTAMINANTS*

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The presence of radioactive contaminants in cholesterol-C^{14} prepared via isolated organ perfusion or intact animal experiments has been well established by Schwenk and associates (1952, 1953), although the nature of these materials remains obscure. In that these impurities are intimately associated with cholesterol and exhibit high specific activities relative to cholesterol a precursor relationship has been suspected (Schwenck and Werthessen, 1952).

The radioactive contaminants persist in cholesterol precipitated as the digitonide. Cholesterol regenerated from the digitonide retains the extraneous activity, even after several recrystallizations or after chromatography. Successive chromatography of contaminated cholesterol on silica gel and assay of each benzene-eluted fraction permitted separation of radiochemically pure cholesterol together with cholesterol fractions of higher specific activities (Smith and Werthessen, 1953). Purification via the dibromide has been advocated by Schwenk and Werthessen (1952) as the most direct route to radiochemically pure cholesterol prepared in biosynthesis.

A recent report by Dayton, Mosbach, and Kendall (1953) offers evidence that 7β-hydroxycholesterol, 7-ketocholesterol, cholestane-3β, 5α, 6β-triol, or squalene do not contribute significantly to the digitonin-precipitated impurities accompanying cholesterol-C^{14}. Evidence presented herein obtained from paper partition chromatography of cholesterol-C^{14} preparations support these observations.

Experimental

Biosynthesis Experiment. The liver of a mature sow was perfused with one liter of sow's blood—White's solution (Werthessen and Schwenk, 1952). So-

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dium acetate–1–C^{14} (0.9 mC., prepared from barium carbonate –C^{14}) was injected into the perfusate, which was circulated for 10 minutes. After perfusion the liver, weighing 860 g., was hydrolyzed in 30 per cent potassium hydroxide solution and the unsaponifiable fraction isolated by pentane extraction. The washed extract was reduced in volume under nitrogen, and the residue dissolved in methyl alcohol (500 ml. volume, weight of residue: 2.548 g.). Portions of this solution were withdrawn for the several analyses.

Cholesterol digitonide was precipitated in the usual manner and washed with 90 per cent ethyl alcohol, acetone, acetone-ether (1:1), and ether. The digitonides were first dried in air, then in an oven at 110° and weighed. Following decomposition with pyridine the regenerated cholesterol was assayed for radioacticity. Purification via the dibromide was accomplished according to the method of Schwenk and Werthessen (1952).

Paper Chromatography. Portions of the liver extract were subjected to paper partition chromatography using the β-phenoxyethyl alcohol-heptane system of Neher and Wettstein (1952). Sheets of Whatman No. 1 filter paper from 4 to 15 cm. in width and 57 cm. long were marked with 3 mm. wide lanes parallel to and 9 cm. from the shorter side of the paper, dipped into a 10 per cent acetone solution of β-phenoxyethyl alcohol, and dried momentarily in air. The liver extract was applied to the paper within the marked lane from a micro pipette, each application dried with a jet of nitrogen prior to further additions. The amounts of solution applied varied from 1.0 ml. to 5.0 ml., containing 5.1 to 25.5 mg. of solids. The prepared papers were irrigated in sealed glass cylinders at 20° with heptane previously equilibrated with β-phenoxyethyl alcohol. Two to four hours irrigation afforded adequate separations of sterol from other lipids present.

Two millimeter strips were cut from either side and from the middle of the air-dried chromatogram and dipped into the Liebermann-Burchard reagent of Kingsley and Schaffert (1949). The sterol zone so located was marked on the main chromatogram and this portion was excised and cut into small pieces and extracted with chloroform. The chloroform extract evaporated under nitrogen yielded crystalline material which was contaminated with small amounts of β-phenoxyethyl alcohol. Recrystallization from methyl alcohol removed this impurity, giving cholesterol samples for analysis.

Examination of the dried chromatogram in ultraviolet and visible light permitted location of certain other lipid zones. For comparison purposes the tissue extract was also chromatographed in a reversed phase chromatographic system described by Kritchevsky, *et al.* (1950, 1952b). Whatman No. 1 paper was impregnated with a 2 per cent "Quilon" solution and irrigated with redistilled methyl alcohol. After 2.5 hours the papers were removed from the tanks and the solvent front marked. The lipid zones were located on the dried chromatograms as previously described.

Radioactivity Assay. Cholesterol was plated onto disks as previously described (Smith and Werthessen, 1952). Assays were performed with a gas-flow instrument with a Tracerlab 100 scaler. Sufficient counts were totalled to assure a counting error of less than 5 per cent. Specific activities were corrected empirically for self-absorption.

Results

The data in table 1 summarizes the analyses performed on the liver extract using the conventional digitonin precipitation and isolation methods. Digitonin-precipitable substances, calculated as cholesterol, comprise 54 per cent of the nonsaponifiable fraction, or 0.16 per cent of the wet liver. The specific activity of the sterol fraction averaged 750 counts/minute/mg. in these and other analyses on the extract. The nonsaponifiable fraction counted 1,900 counts/minute/mg. Cholesterol of constant specific activity, purified via the dibromide, counted 310 counts/minute/mg.

TABLE 1

Analysis of Liver Extract via Digitonin Precipitation

	(1)	(2)	(3)
Weight nonsaponified fraction, mg.	85.2	85.2	85.2
Weight digitonide, mg.	189.0	180.8	185.9
Calculated sterols, per cent	55.5	53.0	54.6
Specific activity, c./min./mg.	799	718	730
Melting point*, after recrystallization	148.0–149.0°	147.5–148.5°	148.0–149.0°
Specific activity, after one recrystallization (MeOH)	530	392	528

* Kofler block, corrected.

In this particular liver extract preparation 59 per cent of the radioactivity (as measured by specific activity) of the digitonin-precipitable fraction is not due to cholesterol. The data of table 2 were obtained on ten separate paper chromatographic analyses of the liver extract. The average specific activity of the eluted and recrystallized cholesterol is 700 counts/minute/mg., within the limits of the radioactivity assay the same as that of the digitonin-precipitated fraction.

Figure I is a representative paper chromatogram of the liver extract. Of the several lipid materials present, cholesterol was the most radioactive. No other Liebermann-Burchard reactive sterols (or reactive towards antimony trichloride) were detected.

Discussion. The results of Dayton, Mosbach, and Kendall (1953) demonstrate that the autoxidation products of cholesterol: 7β-hydroxycholesterol, 7-ketocholesterol, and cholestane-

3β, 5α, 6β-triol do not contribute significantly to the radioactive contaminants of biosynthetic cholesterol. The nonsaponifiable fraction from perfused liver likewise does not contain these materials. 7α-Hydroxycholesterol, 7β-hydroxycholesterol, and 7-ketocholesterol were not detected with antimony trichloride or the Liebermann-Burchard reagents. These sterols separate well from cholesterol in the solvent system employed (mobility relative to cholesterol, R_C 0.17, 0.11, and 0.15, respectively) (Smith, 1954).

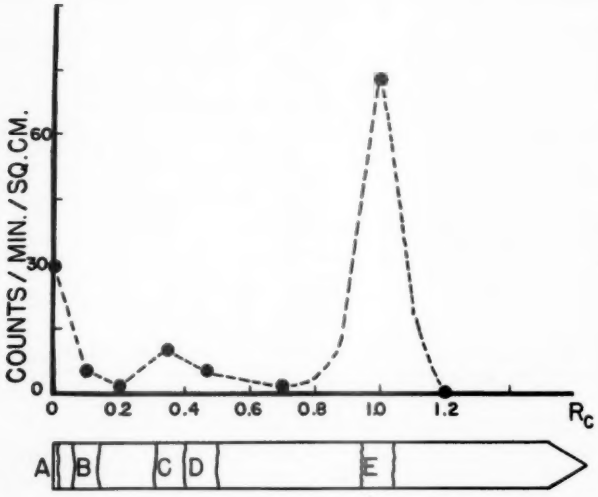

FIG. I. Typical chromatogram of liver extract using the β-phenoxyethyl alcohol-heptane system of Neher and Wettstein (1952). The zones are characterized: Zone A, starting line, yellow-green UV fluorescence, brown color with Lieberman-Burchard, R_C=0, R_F (Quilon)=0; Zone B, yellow UV fluorescence, brown color with Liebermann-Burchard, R_C=0.10, R_F (Quilon)=0.91; Zone C, blue UV fluorescence, R_C=0.35, R_F (Quilon)=0.85; Zone D, yellow visible, R_C=0.47; Zone E, sterol zone, violet color with Liebermann-Burchard reagent, R_C=1.00, R_F (Quilon)=0.54.

Cholestane-3β, 5α, 6β-triol was not detected (R_C 0.02), although in this respect some uncertainty must remain since yellow pigments overlapped the area normally occupied by the triol. Similarly cholestane-3β, 5α-diol-6-one was not detected.

Both of these sterols would have been detected in their expected position on the chromatogram had they been present in concentrations exceeding 10–20 μg./sq. cm. even though pigments did overlap the zones. It has been suggested that cholestane-3β, 5α, 6β-triol may occur in certain cholesterol preparations from perfusions, in that an oxidation product, cholestane-3β, 5α-diol-6-one has been isolated from the sterol sample (Schwenk, Werthessen, and Rosenkrantz, 1952).

No evidence was found for the presence of the hydrocarbon, squalene, in the liver extract or in the digitonin nonprecipitated fraction therefrom. Exposure to iodine vapors (Kritchevsky and Kirk, 1952a; Dauben, *et al.*, 1952) or the Liebermann-Burchard reagent failed to detect squalene (R_C 1.25).

TABLE 2

Paper Partition Chromatographic Analyses of Liver Extract

Chromatogram No.	Weight eluted, recrystallized, mg.	Melting point*	Specific activity, counts/minute/mg.
From liver extract directly:			
B-38	9.7	.....	873
B-43	3.9	144–146°	480
B-44	6.9	144–145°	1018
B-45	15.9	148.0–149.5°	984
B-70	4.7	147.0–149.5°	487
B-71	4.5	147.0–148.0°	505
B-72	3.2	145–146°	536
B-91	5.0	143.5–145.5°	673
B-92	2.9	148.0–149.0°	694
B-93	6.2	145–148°	760
From digitonized cholesterol:			
B-49	7.7	.....	555
From cholesterol via dibromide:			
B-94	4.7	149.0–149.5°	322
B-95	8.6	148.0–149.0°	310
B-96	..	147.0–148.0°	310

* Kofler block, corrected.

Although none of these substances was present in amounts detectable by conventional color reagents their presence in quantities exceeding the sensitivity of the tests applied is not excluded. That these materials contribute to the radioactive contaminants of cholesterol is unlikely, since adequate separations are achieved in the paper partition system used. The presence

of the radioactive contaminants in about the same amounts (as measured by specific activity of eluted cholesterol) as are present in samples isolated via the digitonide is indicative that other unidentified substances are the contaminants.

The question of cholestan-3β-ol, Δ^7-cholesten-3β-ol (lathosterol), and 7-dehydrocholesterol cannot be answered conclusively from these experiments. Cholesterol eluted from the chromatograms and recrystallized was free from β-phenoxyethel alcohol and exhibited a good melting point. The presence of Δ^7-cholesten-3β-ol and cholestan-3β-ol could not be detected in the preparations by infrared analysis.[1] Although Δ^7-cholesten-3β-ol and 7-dehydrocholesterol are inseperable from cholesterol under the conditions of the experiments,[2] the presence of these sterols can be detected by their characteristic color behavior with the Liebermann-Burchard reagent or other reagents. In the presence of large amounts of cholesterol these selective color tests are inconclusive. Use of the "Quilon" reversed phase paper chromatographic system of Kritchevsky (1950, 1952b) does permit separation of cholesterol from 7-dehydrocholesterol. Again overlapping of yellow pigments interfere with interpretations of chromatograms of the liver extract, Zones B and C (Fig. I) of R_F=0.91, 0.85, respectively, overlapping the 7-dehydrocholesterol location, R_F 0.86.[3] Examination of the digitonin precipitated sterols alleviates this objection, the interfering pigments being nonprecipitable. No good evidence was found for the presence of either of these sterols in the eluted cholesterol.

The presence of radioactive contaminants in cholesterol eluted from the chromatograms of liver extract indicates that the substances comprising the impurities have identical mobility with cholesterol on the paper partition system. These impurities are retained in the cholesterol samples following digitonin precipitation and regeneration, and after chromatography on paper;

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[1] The infrared analysis was conducted on a carbon disulfide solution (1 mg./ml.) of once recrystallied cholesterol. The authors are indebted to Dr. Harris Rosenkrantz of the Worcester Foundation for Experimental Biology, Shrewsbury, Mass., for these analyses.

[2] Cholesterol, Δ^6-cholesten-3β-ol, Δ^7-cholesten-3β-ol (lathosterol), $\Delta^{8(14)}$-cholesten-3β-ol and cholestan-3β-ol are inseparable in the Neher-Wettstein system, the "Quilon" system, and in a variety of other solvent systems examined.

[3] Kritchevsky and Kirk (1952b) report cholesterol R_F=0.55, 7-dehydrocholesterol R_F=0.88.

however, cholesterol purified to constant specific activity via the dibromide did not show an altered specific activity after paper chromatography.

The agreement between the average specific activities of the eluted and recrystallized cholesterol and the digitonide-regenerated cholesterol is probably fortuitous. Recrystallization from methyl alcohol alone causes variation in the specific activity of cholesterol obtained from the digitonide (Table 1). It is likely that the activities measured in the eluted cholesterol are due to different mixtures of substances than those measured on digitonin-precipitated cholesterol.

Separation of cholesterol from nonsaponifiable, nonsterol materials is illustrated in figure I. The liver extract contains several yellow pigments which fluoresce in ultraviolet light and give brown colors with the Liebermann-Burchard reagent. A blue fluorescent substance (R_C 0.35) was separated from these yellow pigments and from cholesterol. These lipid materials were not precipitated with digitonin and contained significant radioactivity as measured on square centimeter pieces of the paper chromatogram. Using the "Quilon" system the location of the yellow pigments and the blue fluorescent zone were reversed relative to that of the cholesterol.

Summary. Isolation of cholesterol from the nonsaponifiable fraction of a prefused swine liver by paper partition chromatography has permitted certain conclusions to be drawn as regards the nature of the radioactive contaminants of cholesterol-C^{14} of biosynthetic origin. In these experiments 59 per cent of the radioactivity precipitated as an insoluble digitonide was not cholesterol. The radioactive impurities were not removed from the cholesterol on paper chromatography. From the data presented 7α-hydroxycholesterol, 7β-hydroxycholesterol, 7-ketocholesterol, cholestane-3β, 5α, 6β-triol, cholestane-3β, 5α-diol-6-one, or squalene do not constitute appreciably the radioactive impurities co-precipitated with biosynthetic cholesterol by digitonin.

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PLANT EXTRACTS TESTED AGAINST EGG CULTIVATED VIRUSES*

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A research project is under way at The University of Texas into the possibilities of discovering therapeutic agents in plant extracts (Taylor, *et al.*, 1951; Taylor, *et al.*, 1952; Burlage, *et al.*, 1952). Plants with known biological effects have been given priority in the initial stages of the work. The chief objective has been to test the effectiveness of the various extracts against mouse- and egg-grown tumor transplants, but parallel with the cancer therapy aspect of the investigation, the various plant materials have been checked for antibacterial and antiviral activity. The present paper is concerned with the results obtained in the virus work. The tumor and bacterial data are considered in other reports.

Nearly all virus-induced infections are as yet refractory to treatment by chemotherapeutic agents. The comparatively large viruses of the psittacosis group are affected by some of the current antibiotics, but aside from these, the chemotherapy of virus diseases is still in the pioneer stage.

In a recent paper, McLean, Jr. (1952) has described the use of egg adapted viruses for the rapid screening of materials for their possible antiviral activity. He pointed out that by the utilization of a few representative viruses it was possible to make a rapid preliminary evaluation of a substance with respect to its antiviral properties.

Seed viruses of yolk sac adapted strains used in the screening program at the Parke, Davis & Company laboratories were obtained through the courtesy of Dr. I. W. McLean, Jr., for use in testing the various plant materials. To date, extracts from 44 different plants (Table 1) have been tested on each of the 4 viruses used in this work.

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* From the Biochemical Institute and Pharmaceutical Foundation of The University of Texas and the Clayton Foundation for Research, Austin, Texas.
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Materials and Methods

Yolk sac adapted viruses used in the tests were: vaccinia, N. Y. State Health Dept. line; influenza, Lee Strain NIH mouse line; eastern equine encephalomyelitis, Massachusetts strain; and ornithosis, meningopneumonitis (Francis) strain. Eggs were inoculated as follows: meningopneumonitis on the 6th day of incubation at a concentration which killed the embryos in 4–5 days, vaccinia on the 9th day of incubation at a concentration which killed the embryos in 3–4 days, influenza on the 8th day of incubation at a concentration which killed the embryos in 2–3 days, and encephalitis on the 10th day of incubation at a concentration which killed the embryos in 1 day.

TABLE 1

Plants Used in the Virus Tests

Acolypha phleoides Cav.[1]	Oleander sp.
Acocotillo o feurogreco[1]	Papaya sp.
Allium helleri Small	*Parthenium hysterophorus* L
Ambrosia aptera DC	*Phoradendron flavescens* (Pursh) Nutt.
Amphyteryngium abstringens[1] (Schlect) Schiede	*Poinciana pulcherrima* L
Anemone decapetala Ard	Piscaria sp.
Asclepias decumbens L	*Leucophyllum frutescens* (Berl.) Johnston
Astragalus Eerlei Greene	Rhubarb root[3]
Astragalus Wootoni Sheldon	*Rhus glabra* L
Cannabis sativa L	Salvia sp[1]
Chicorium intybus L	*Salvia farinacea* Benth.
Cnidoscolus texanus (Muell. Arg) Small	*Salvia Greggi* A. Gray
Cola de caballo[1]	Sarsaparilla sp[3]
Conium maculatum L[2]	Sensitive Plant
Cooperia pedunculata Herb.	*Sophora secundiflora* (Ortega) Lag.
Datura	*Tecoma radicans* (L) DC Seem
Gossypium Sp.	*Toxicodendron radicans* L.
Gutierrezia texana T and G	*Verbena bipinnatifida* Nutt.
Delphinium sp.[3]	*Vitis candicans* Englem.
Liatris punctata Hook	*Yucca arkansana* Trel
Lupinus texansis Hook	*Zephranthes texana* Herb
Melia Azedarach L	
Nicotiana glauca Graham	

[1] Obtained from and identified by a Mexican herbalist.

[2] Obtained by Mr. Jack L. Groff, County Agent in Bandera County.

[3] Obtained from S. B. Penick & Co., New York, N.Y.

The test materials, in 0.2 cc. quantities, were injected over the blood vessels of the area vasculosa by a technique previously described (Taylor, 1950; Taylor, 1953). The control groups were treated in the same manner with distilled water. The time of the introduction into the egg of a particular plant material varied in accordance with the time required for the virus to kill the embryo. The test extract was injected on the 3rd day after meningopneumonitis, the 2nd day after vaccinia, and 6 hours after egg inoculation with influenza and encephalitis. Each experiment of the preliminary series

was set up with 20 eggs as controls and 5 eggs in each experimental group. Retests with extracts which gave positive results in the preliminary series were usually carried out with 20 eggs in the control and 10 eggs in the experimental groups. The eggs were candled twice daily and survival time for experimental groups was calculated as a percentage of the survival time of the controls. The seed viruses for a particular experiment were obtained from eggs previously inoculated and preserved at deep freeze temperature after the death of the embryos.

The processing of the plant material and preparation of the various extracts has been described previously (Taylor, *et al.*, 1951; Taylor, *et al.*, 1952; Burlage, *et al.*, 1952). As a general routine, extracts were prepared with the following solvents: cold water, pH 6.6, 7.0, and 7.4; boiling water, pH 7.0; absolute alcohol; and acetone. The various solutions were reduced to a dry weight basis by low temperature, high vacuum dessication and redissolved at suitable concentrations for trials against the viruses. The toxicity of each extract was checked by its effect at relatively high concentrations on mice and chick embryos. Mice were given five consecutive daily injections and autopsied on the 7th day after the initial treatment. Embryonated eggs were given single extract injections over the blood vessels of the area vasculosa on the 11th day and autopsied on the 16th day of incubation. Control mice and eggs received injections of distilled water. Varying degrees of weight loss and atypical visceral manifestations with special reference to the liver and spleen were considered to be evidence of toxicity for a particular dosage. The extracts were used in the tests against the viruses in concentrations which were well tolerated by both mice and embryos.

Results

The results are summarized in table 2. It will be noted that 22 of of the 44 plants used in this study gave extracts which increased the survival 20 per cent, or more, of the chick embryos infected with one or other of the 4 viruses. Extracts from 10 plants were effective in this respect against vaccinia, 6 against meningopneumonitis, 10 against influenza, and 4 plants gave extracts with some protective effect against encephalitis. Three of the plants gave extracts which had a 20 per cent or more deterrent effect against 3 of the 4 viruses used. Trials which resulted in less than a 20 per cent extension of life for the infected embryos are included in the table when one or more extracts from the same plant were in the higher range of effectiveness, since these results tended to increase the possibility that an effective compound was associated with a particular plant species. *Allium helleri*, for example, gave extracts which increased the survival of embryos infected with vaccinia, influenza, and encephalitis 31 per cent, 53 per cent, and 45 per cent, but in addi-

TABLE 2

The Effect of Plant Extracts on the Survival of Virus Infected Chick Embryos. (Average of Two or More Tests)

Plant	Solvent	Survival Control = 100			
		Vaccinia	Pneumonitis	Influenza	E.E.E.
Allium helleri Small	Acetone	131	...	...	145
	H_2O pH 7.0	...	112	...	...
	H_2O pH 7.0 boiled	...	108	153	...
Ambrosia aptera DC	Abs. Alsohol	111	118	...	...
	H_2O pH 6	...	117	...	120
	H_2O pH 7	139	122	...	108
	H_2O pH 7 boiled	142	...	...	109
Amphypteryngium abstringens (Schlect) Schiede	Abs. Alcohol	120	...	...	...
Anemone decapetala Ard	H_2O pH 7	141	...	...	...
	H_2O pH 7 boiled	152	...	...	...
Asclepias decumbens L	Acetone	...	131	...	...
Cannabis sativa L	Abs. Alcohol	138	...	...	...
	H_2O pH 6	...	129	...	...
	H_2O pH 7 boiled	...	119	...	...
Conium maculatum L	Acetone	127	...	...	...
	Abs. Alcohol	112	...	...	...
	H_2O pH 6	...	...	118	115
	H_2O pH 7 boiled	140	...	...	...
Datura sp.	Acetone	144	...	...	...
	H_2O pH 7	...	...	...	...
	H_2O pH 7 boiled	132	115	...	...
Gutierrezia texana T and G	H_2O pH 6	112	...	...	...
	H_2O pH 7	...	...	158	...
	H_2O pH 7 boiled	...	...	...	...
	H_2O pH 8	128	...	...	...
Liatris punctata Hook	Abs. Alcohol	...	120	...	...
	H_2O pH 8	...	115	...	...
Nicotiana glauca Graham	H_2O pH 8	...	...	132	...
Papaya sp	Abs. Alcohol	...	...	...	120
	H_2O pH 7	116	...	...	...
	H_2O pH 8	...	...	117	...
Parthenium hysterophorus L.	Acetone	...	...	111	...
	Abs. Alcohol	...	...	128	...
	H_2O pH 6	...	...	124	...
	H_2O pH 7	...	...	114	...
Poinciana pulcherrima L.	H_2O pH 6	113	...	144	...
Rhubarb Root	Acetone	...	113	...	...
	H_2O pH 7	...	...	134	...
	H_2O pH 7 boiled	...	...	152	...
	H_2O pH 8	115	...	...	...

TABLE 2—(Continued)

The Effect of Plant Extracts on the Survival of Virus Infected Chick Embryos. (Average of Two or More Tests)

Plant	Solvent	Survival Control = 100			
		Vaccinia	Pneumonitis	Influenza	E.E.E.
Rhus glabra L.	H_2O pH 7	...	...	122	...
	H_2O pH 8	...	...	109	...
Salvia farinacea Benth	Abs. Alcohol	109	...	...	...
	H_2O pH 6	132	...	...	...
Salvia Greggi A Gray	H_2O pH 6	...	107	...	...
	H_2O pH 7 boiled	...	121	...	...
Sarsaparilla sp.	H_2O pH 7 boiled	...	...	138	...
Sensitive Plant	Abs. Alcohol	...	...	130	...
	Acetone	...	...	130	...
	H_2O pH 5.98	131	...	...	...
	H_2O pH 8.01	...	...	...	120
Verbena bipinnatifida Nutt.	Acetone	...	...	120	...
	H_2O pH 6	113	...	109	...
	H_2O pH 7	...	...	...	...
Yucca arkansana Herb.	Abs. Alcohol	...	161	...	...

tion was effective with meningopneumonitis in this respect 8 per cent and 12 per cent for 2 extracts.

Discussion. The results obtained to date have not been of a spectacular nature but do indicate that plants may be a source of antiviral compounds. When separate crude extracts of a particular plant manifest some inhibitory action against three different types of viruses, there is the possibility that chemical purification procedures would disclose a substance of value in virus chemotherapy. This is especially so since the various extracts were used at levels well below that which produced toxic effects in mice or chick embryos.

It is recognized that materials which gave the chick embryo some protection against a particular virus would not necessarily do the same for a virus-infected free animal. However, since the extracts were introduced into the egg over the chick membranes, the protective effect against virus action was mediated through the embryo blood stream which is comparable to a subdermal injection in a free-living animal. Also, the injection of the test substance into the eggs was delayed until there had been suffi-

cient time for the inoculated virus to be taken up by the chick tissues, but before there was any evidence of virus toxicity.

A search through the literature has not disclosed reports of the use of plant extracts in virus chemotherapy studies. The chief emphasis, at present, appears to be on the search for antibacterial substances. Also, relatively few university laboratories are in a position to carry on a program involving the evaluation of compounds with respect to possible antiviral properties. In addition, the lack of success which, in the past, has attended investigations in this field has not made it an attractive research project. It does seem, however, that the time has come for a more concerted attack on virus chemotherapy. The increasing evidence that malignant growths may be due to this type of infective agent increases the importance of such research. Our experience to date encourages us to believe that the patient "screening" of plant extracts through a test program is one approach to the problem.

Summary

Egg adapted strains of vaccinia, influenza, meningopneumonitis, and encephalitis were used to test the antiviral activity of a series of plant extracts. Embryonated eggs were virus-infected through the yolk sac. The plant extracts were introduced over the blood vessels of the area vasculosa at dosages well tolerated by mice and 11-day chick embryos. Twenty-two of the 44 plants tested furnished extracts which prolonged the life 20 per cent or more of chick embryos infected with one or other of the 4 viruses. In 3 instances, individual plants gave extracts which had a deterrent effect on 3 of the 4 viruses used in the tests.

We wish to thank Dr. C. C. Albers, Professor of Pharmacognosy, in the College of Pharmacy, and Dr. Benjamin C. Tharp, Professor of Botany, for assisting us in the identification of many of these plants.

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CALLING ATTENTION TO:

Items of Possible Interest to Our Friends

May, 1954

Association Proceedings: 300 reports, 52 demonstrations, & 11 movies were presented at the meeting of the American Association of Anatomists at The Univ of Texas Medical Branch, Galveston, April 7, 8 & 9, 1954 (*Anat Rec* 118/275–443/54), and 23 reports were offered at the Tissue Culture Association session, April 6, with many informal demonstrations (*ibid* pp 444–454). Papers offered at the meeting of the American Association of Plastic Surgery at The Univ of Texas Medical Branch, Galveston, April 21–23, will appear in the *Journal of Plastic & Reconstructive Surgery*. The discussions at the Josiah Macy, Jr., Foundation Conference on "Medical & Psychological Team-work in the Care of the Chronically Ill," held at The Univ of Texas Medical Branch, Galveston, March 28–31, will appear in *Texas Reports on Biology & Medicine*, & separately in book-form.

General: E Bossi does well with mother-child relationship in *Child with a Flower* (Macmillan, NY, 54, 205pp, $3.75) . . . L Carmichael edits 2nd ed of *Manual of Child Psychology* (Wiley, NY, 54, 1295pp, $12) . . . G Carson revives *The Old Country Store* (Oxford Univ Press, NY, 54, 330pp, $5) . . . HS Commager clarifies *Freedom, Loyalty, Dissent* (Oxford Univ Press, NY, 54, 143 pp, $2.5) . . . PR Heyl says gravitation is still a mystery (*Sci Month* 78/303/54) . . . H Loewe describes long scientific collaboration of P Ehrlich (1854–1915) & E von Behring (1854–1917) (*Arzn-Forsch* 4/107/54) . . . AH Maslow discusses instinctoid nature of basic needs (*J Personal* 22/326/54) . . . DW Maurer & VH Vogel analyse *Narcotics & Narcotic Addiction* (CC Thomas, Springfield, Ill, 54, 320pp, $7.5) . . . VJ McGill goes into *Emotions & Reason* (CC Thomas, Springfield, Ill, 54, 122pp, $3.5) . . . E Olson analyzes *The Poetry of Dylan Thomas* (Univ Chicago Press, 54, 164pp, $3.25) . . . RJ Pumphrey is witty on the evolution of thinking (*Brit J Philos Sci* 4/315/54) . . . E Quastler edits *Information Theory in Biology* (Univ. Illinois Press, Urbana, 54, 234pp, $4) . . . G Sarton appraises *Galen of Pergamon* (Univ Kansas Press, Lawrence, 54, 124pp, $2.5) . . . FC Sheridan's *Galveston Island* (in 1839) is edited by WW Pratt (Univ Texas Press, Austin, 54, 223pp, $3.5) . . . LW Terman completes study of gifted children in *The Gifted Child Grows Up* (Stanford Univ. Press, Stanford, Calif. 54, 448pp, $6) . . . P Tillich doubles in *Love, Power & Justice* (Oxford Univ Press, NY, 54, 118pp, $2.5) . . . & *The Courage to Be* (Yale Univ Press, New Haven, Conn, 54, 208pp, $3).

Clinical: FP Chinard & Co contribute symposium on nephrotic syndrome (*J Clin Invest* 33/621–723/54) . . . CN Frazer & IH Blank offer *A Formulary for External Therapy of the Skin* (CC Thomas, Springfield, Ill, 54, 118pp, $3) . . . FM Hanger discusses meaning of results of liver function tests (*Am J Med* 16/565/54) . . . HS Kaplan & SJ Robinson consider *Congenital Heart Disease* (McGraw-Hill, NY, 54, 126pp, $12.5) . . . M Kitaoka & Co indicate

glutamyl-choline & thiamine may aid in polio (*Jap J Med Sci Biol* 6/475/53) . . . RJ Popkin notes subjective disability in post-phlebitic syndrome (*Calif Med* 80/291/54) . . . M Prinzmetal & Co continue studies on mechanism of ventricular action (*Am J Med* 16/469, 490/54) . . . W Raab & W Gigee find excretion of conjugated catechols decreased in uremia (*Circulat* 9/592/54) . . . S Sriranachari & MD Ananthachari indicate relation between malnutrition & liver cirrhosis (*Indian J. Med Sci* 8/31/54).

PRECLINICAL: PK Bondy suggests pituitary growth hormone promotes fat use as metabolic substrate, aiding storage of protein & carbohydrate as protoplasm & glycogen (*Yale J. Biol Med* 26/263/54) . . . AP Fishman finds constant respiratory dead space (av 164 ml) over wide range of conditions (*J Clin Invest* 33/469/54). . . DL Fox writes on *Animal Biochromes & Structural Colours* (Cambridge Univ Press, NY, 54, 379pp, $11) . . . H Gest analyses oxidation of molecular hydrogen by microorganisms (*Bact Rev* 18/43/54) . . . G Hodgson & J Toha find hemorrhagic anemic plasma or urine increases red cell & hemoglobin production (*Blood* 9/299/54) . . . GB Koelle edits symposium on neurohumoral transmission (*Pharmacol Rev* 6/3–131/54) . . . BW Meade shows salicylic acid only of various hydroxybenxoic acids, increases oxygen consumption (*Ann Rheum Dis* 13/61/54) . . . B Nisman surveys LH Strickland reaction (*Biochem J* 28/1746/34) of coupled deamination (*Bact Rev* 18/16/54) . . . EL Oginsky & WW Umbreit offer *An Introduction to Bacterial Physiology* (WH Freeman, San Francisco, 54, 512pp, $6) . . . KH Schmidt considers mutagenic effects of triethylenephosphoramides & mercaptopurins (*Arzn-Forsch* 4/146/54).

JUNE, 1954

PRECLINICAL: RM Anderson proposes study of intrinsic blood pressure, i.e., pressure in vascular system as a whole (*Circulation* 9/641/54) . . . M Burnet & PE Lind consider genetics of virulence in influenza viruses (*Nature* 173/627/Apr 3/54) . . . J Dorner analyses vascular dilation with adrenalin (*Arch Exp Path Pharmakol* 221/286/54) . . . AF Essellier & Co analyse mechanism of glucocorticoid eosinopenia (*Blood* 9/531/54). BE Etsten & TH Li recommend dye dilution method of estimating cardiac output without telling what the dye is (*Anesth* 15/217/54) . . . H Friebel & Co consider biochemorphology of phenothiazin derivatives as antiallergic agents (*Arznei Forsch* 4/171/54) . . . J Harley-Mason shows tryptophane synthesis from indole & serine with pyridoxal PO_4 as co-enzyme (*Experientia* 10/134/54) . . . K Hofman begins discussion of *Imidazole & Its Derivatives* (Interscience, NY, 54, 447pp, $13.5) . . . JE Lovelock notes physical instability of red cell membrane in thermal stress (*Nature* 173/659/Apr 10/54) . . . JF Manery analyses water & electrolyte metabolism (*Physiol Rev* 34/334/54) . . . P Muller reviews biochemorphology of insecticides, with reference to Geigy contributions in pest control, hygiene & agriculture (*Experientia* 10/91/54) . . . L Postman & B Conger find speed of recognition of words varies with verbal habits associated with such stimuli (*Science* 119/671/May 14/54) . . . S & AH Robinson review chemical composition of sweat (*Physiol Rev* 34/202/54) omitting reference to citric acid (*Am J Physiol*

63/540/23) . . . S Rothman meets long need in *Physiology & Biochemistry of the Skin* (Univ Chicago Press, 54, 768pp, $19.5) . . . DC Stewart & PL Kirk are optimistic about controlled synthetic media for tissue culture (*Biol Rev* 29/119/54) . . . JEP Toman & JD Taylor describe metabolism of anti-epileptic drugs (*Arznei Forsch* 4/175/54) . . . S Zuckerman considers brain secretions in relation of hypothalamus to pituitary (*Lancet* 266/739/Apr 10/54).

CLINICAL: SA Borgstrom recommends estrogens & androgens with calcium for treating essential hypertension (*Acta Med Scand*, Suppl 290, Stockholm, 54, 70pp) . . . JB Duguid differentiates two types of atherosclerosis, with diet of possible significance in coronary occlusion by thrombosis (*Lancet* 266/891/May 1/54) . . . DP Earle edits symposium on epidemic hemorrhagic fever (*Am J Med* 16/617–709/54) . . . MJ Fiese & Co find ethyl vanillate promising in coccidioidomysosis (*Calif Med* 80/349/54) . . . M Fleishman fails to correlate clinical diagnoses with Szondi syndromes (*J Consult Psychol* 18/89/54) . . . L Lofgren & A Isotalo find repeated histamine shock may deplete thyroid as well as cause gastric ulceration (*Ann Med Exp Biol Fenn* 32/56/54) . . . HB Molish & Co find Blacky Picture test (GS Blum & HF Hunt, *Psychol Bull* 49/238/52), useful in screening naval recruits (*Am J Orthopsych* 24/164/54) . . . HB Peck chairs discussion on group effort in psychotherapy (*ibid* p 128) . . . JB Rotter & Co validate incomplete sentence blank (*J Consult Psychol* 18/105/54) . . . ER Trethewie discusses basic aspects of allergy (*Med J Austral* 1/388/Mch 13/54).

GENERAL: C Bowen describes etiology of teenagers' delinquency in *They Went Wrong* (McGraw-Hill, NY, 54, 218pp, $3.5) . . . WB Cameron & TC McCormick review concepts of security & insecurity (*Am J Soc* 59/556/54) . . . G Carson memorializes *The Old Country Store* (Oxford Univ Press, NY, 54, 350pp, $5) . . . HS Commager rallies around *Freedom, Loyalty, Dissent* (Oxford Univ Press, NY, 54, 141pp, $2.5) . . . JA Curran eulogizes John Shaw Billings (1838–1913) as medical genius (*Bull Med Libr Asso* 42/163/54) . . . J Evans case-history analyses *Three Men* (Knopf, NY, 54, 296pp, $3.75) . . . WB Huie raises disturbing questions in *The Execution of Private Slovik* (Duell, Sloan & Pearce, NY, 54, 246pp, $3.5) . . . G Kahlson edits memorial tributes to Torsten Thunberg (1872–1952) (*Acta Physiol Scand*, 30/Suppl 111/Lund, 53/214pp) . . . K Kundnatitz reviews contributions of CF von Pirquet (1874–1929) to theory of allergy (*Wien Klin Wchnschr* 66/217/Apr 2/54) . . . SP Lucia does well in *Wine as Food & Medicine* (Blakiston, NY, 54, 149pp, $3) . . . C Mayer goes *In Quest of a New Ethics* (Beacon Press, Boston, 54, 212pp, $4) . . . JA Schumpeter gives posthumous *History of Economic Analysis* (Oxford Univ Press, NY, 54, 1260pp, $17.5) . . . HR Stuck, ES McCartney & Co confer on scientific writing (*Science* 119/522–539/Apr 23/54) . . . N Thomas considers *The Test of Freedom* (Norton, NY, 54, 211pp, $3) . . . F Wertham condemns the "comics" in *Seduction of the Innocent* (Rinehart, NY, 54, 221pp, $4) . . . E Wilson well reviews *Genesis* (*New Yorker*, May 15/54, p 117) . . . JD Williams offers *The Compleat Strategyst: a Primer on the Theory of Games of Strategy* (McGraw-Hill, NY, 54, 319pp, $5.5).

MEDICAL AND PSYCHOLOGICAL TEAMWORK IN THE CARE OF THE CHRONICALLY ILL

Reports and Discussions of a Conference held March 28–31, 1954, at Galveston, Texas, under the Auspices of the Josiah Macy, Jr., Foundation of New York City, and The University of Texas Medical Branch

EDITED BY MOLLY HARROWER

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PARTICIPANTS IN THE CONFERENCE ON MEDICAL AND PSYCHOLOGICAL TEAMWORK IN THE CARE OF THE CHRONICALLY ILL HELD AT THE UNIVERSITY OF TEXAS MEDICAL BRANCH, GALVESTON, MARCH 28–31, 1954

Top row from left to right: Kuzell, Holbrook, Peck, Garrett, Meyers, Berry, Mrs. Smart, Miss Steding, Rottersman, Calvin, Lhamon, Leake, Huston, McDanald. *Middle row:* Ham, Robbins, Seidenfeld (behind Ham), Jacobsen, Erik Wright, Fremont-Smith, Watson, Davison, Holsopple, Cobb, Shorr. *Bottom row:* Brown, Beatrice Wright, Harrower, Kohn, Kubie, Sanger, Lee.

Participants in the Conference on Medical and Psychological Teamwork in the Care of the Chronically Ill held at Galveston, Texas, March 28–31, 1954, under the joint sponsorship of the Josiah Macy, Jr. Foundation of New York City and The University of Texas Medical Branch, Galveston.

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FOREWORD

FRANK FREMONT-SMITH

The Macy Foundation is very happy to join hands with The University of Texas Medical Branch on the important and timely subject of medical and psychological teamwork in the care of the chronically ill. The Foundation is much interested in the development of multi-professional teamwork and also in the improvement of communication between the members of such a team. One of the problems of communication is the common assumption that if we make a good statement of a problem or an issue, *i.e.*, a statement satisfactory to ourselves, we have in fact made a successful communication. Thus we are inclined to limit our efforts to making statements *at* people and are too little concerned with the adequacy of our communication *with* them. If we find that our statement is not understood at once we are inclined to re-state it in the same words, but louder—and if that fails, we add an angry note! Thus we are too much concerned with our transmitting sets and pay too little attention to the other fellow's receiving set. If there is a distorting lens or a "blind spot" in the receiving set, we gain little by merely increasing the power of our transmitting set.

At our regular conferences we make quite a point of encouraging interruptions, for it is through interruptions and questions raised as soon as they come to mind that the degree of communication between a speaker and the group can be tested, and if necessary, immediately improved by argument and discussion. Otherwise the group may listen politely but fail to understand, or some may misunderstand without realizing their mistake.

One interesting discovery about communication is that it breaks down readily under anxiety and improves with reassurance. We make a point, therefore, of trying to introduce into our conferences large doses of what I like to call "*free floating security*." (This is my own invention and is the antithesis of the "free floating anxiety" we hear bandied about by psychiatrists.) In an atmosphere that is warm and friendly the blind spots begin to shrink and the distorting lenses are lowered or moved aside, and genuine communication begin to take place! In other words,

the members of the group begin to like one another, and friendship and communication go hand in hand. I hope free floating security will be the order of the day!

It has been our experience in many conferences that each of the participants invited comes with varying degrees of assurance that he or she has the right answer to the problem! One of the most important developments during the first part of the conference is to expose ourselves and our ideas to each other so that each of us may see that our own particular perception of the answer does not exactly fit with the preconceptions of the others, that the ideas we brought with us are not quite good enough, and that we must enlarge our view. Another point that obtrudes itself in nearly every conference is the problem of single causality. The physicists have long ago discarded single causes for a more relativistic viewpoint which recognizes multiple causes or a gestalt. The problem resolves itself not so much as to what is the *true* cause of an event, but rather as to the *relative weight* to be attached to the several discernible causes.

The stage of discovering at the outset of a conference that our own position is not quite tenable, is a stage associated with a certain degree of anxiety, but if this can be compensated by large doses of free floating security then we tolerate the stretching of our minds and the enlargement of our perspectives which is essential to growth.

A final word on the topic of our conference. We shall undoubtedly discuss the care of the chronically ill from many points of view, and I hope we will—but there is one common denominator to which I hope we shall return again and again—and that is: *the needs of the patient.*

THE CONFERENCE: ITS ORIGIN, PLAN AND PROCEDURE

MOLLY HARROWER

The idea to hold a conference on problems relating to medical and psychological teamwork, in the care of the chronically ill, began in discussions between an internist and a psychologist, jointly concerned with two main questions: In the first place, what can be done to assist the general practitioner and the internist to handle effectively the psychological problems which he sees in his chronically ill patients; In the second place, what can be done to obtain the most effective use of the rising and rapidly growing profession of clinical psychology;—what skill does the psychologist have which can be effectively turned in the direction of the handling of patients who are chronically ill;—how can the training of this new professional group be modified so that it includes awareness of the problems of the chronically ill;—how can interprofessional teamwork be developed so that there is less wastage and more advantageous use made of the clinical psychologist in handling the chronically ill.

Initial discussions along these lines suggested the great advantages that might be obtained from wider discussions along inter-disciplinary lines with persons actively interested in various phases of these general problems. In imagination we envisaged inviting others to participate, conjured up a secluded and restful spot where we might meditate and deliberate along with our colleagues. As a final touch, since there is no charge for "free, floating fantasy," we found us a foundation willing to support this enterprise with interest in the channeling and implementing of some of our ideas.

Owing to the interest and generosity of the Josiah Macy, Jr. Foundation, hand in hand with The University of Texas Medical Branch in Galveston, our conference materialized six months after we had envisaged it. Moreover, it turned out to be in actuality better than in anticipation, and our gratitude to those who financed and housed us is very great.

In making these proceedings available to the general public we have enlarged our circle of interested participants considerably

further, and it is our hope that in response to some of the ideas embodied here will come expressions of active interest, and suggestions for the further growth and expansion of this preliminary experimental undertaking.

Those invited to participate in the conference were chosen from the professional ranks of internists, psychiatrists and clinical psychologists. These three disciplines were felt to be quite indispensable in a preliminary discussion of this problem, since "psychological" in its widest sense of necessity requires the contribution of the psychiatrist as well as the clinical psychologist. Therefore, without adequate representation from these fields discussion could not even have been started. To these three disciplines were added physicians interested in hospital administration, since it was our hope that some of our ideas, in terms of teamwork, could be better developed against a background of those with administrative know-how.

Because conferences supported by the Macy Foundation are inevitably limited to a group of under thirty persons, it became clear that much as we might have wished it, representatives from social work, nursing, occupational and physical therapy, and many others could not be included. The focus of this conference was, essentially, the active participation of clinical psychologists in areas where, up to now, for various reasons they had not pulled their full weight. Representatives of too many diverse points of view, although essential to the understanding of the chronically ill patient did not, in this initial conference, seem to rate a high enough priority to offset an inevitable diffuseness that would have been introduced by virtue of the widely represented total picture.

Our conference lasted for three days, opening on a Sunday evening with a festive and delightfully informal supper at the home of Dr. and Mrs. Leake. We got to know each other around the patio supper table, so that meeting at our first general session on the following morning we greeted each other as old friends.

On the opening day, the group met in general session during four, one-hour-and-a-half periods, punctuated by coffee and lunch. During that time each member of the group presented a contribution of approximately ten minutes in duration, concerning which he or she had had ample advance notice during the preceding months. The general areas of interest into which these

initial brief presentations fell have guided us in their grouping in the subsequent pages and they do not necessarily follow in the order of their presentation to the group.

You will find discussions on the broad problem of communicating, organizational problems, papers dealing essentially with sick persons, with chronic illness, with various aspects of team work, of research, on the role that can be played by the clinical psychologist in the medical setting, on problems relating to training of the clinical psychologist.

With some knowledge of what each member would present, the Chairman adopted the procedure in the initial phases of the general discussion, of asking for papers, which fitted in or related to each other, until the whole pattern of the discussion having begun to emerge, our authors volunteered to speak as each felt his contribution was appropriately "cued" by the preceding speaker.

With all our informational cards on the table we were able, on the morning of the second day, to divide into three smaller groups for more intensive discussion in three key areas: One group devoted itself to a more detailed consideration of the problems of "rehabilitation, administration and the relation to national and state agencies"; a second group concerned itself specifically with the problems of training of clinical psychologists, and a third group considered those problems of research to which a combined psychological and medical approach were required. The reports of these three study-groups, and the discussion by the group at large which related to them, are included in the appropriate chapters. Each study group was assigned an expert stenographer provided for us by the Medical Branch. Teamwork at its best was displayed by Miss M. J. Stedding, Mrs. Vicki Smart and Mrs. Pat Liberty.

On the third day of the Conference, each group presented to the Conference as a whole its report for consideration, amplification and discussion. Our time-table allowed approximately two hours for each group to read its report, but general discussion was precipitated by many of the reports, so that our final session turned once more to general interchange of ideas between all members.

Our two festive dinner meetings were followed by speakers of unusual interest. Dr. Truman G. Blocker, Jr., Dean of the Medical Faculty at The University of Texas Medical Branch, fascinated the group with his colored slides and accounts of the, often unrecognized and unsuspected, psychological factors in burns. The next night we were privileged to examine with Dr. Laurence S. Kubie his blueprint for the development of a new paramedical profession, included here as Chapter V.

We encouraged post-conference correspondence, and in such correspondence Dr. Brown raised a question concerning what had seemed to him as an important omission in our discussion. This omission related to the psychologist's role as a psychotherapist, particularly—and this concerned him—outside the framework of some medical setting.

Dr. Brown wrote: "In regard to afterthoughts I have one about the status of the clinical psychologist (as a therapist). It is a practical point which for some reason we actually did not attack."

The whole topic of psychotherapy—and how to make use of the skills of non-medical therapists—probably requires a conference in itself. However, reference to this somewhat controversial matter was omitted in our conference by deliberate design. In planning for the general program the Coordinators felt that there would be amongst the conferees wide differences of opinion in regard to some aspects of this question. We felt that were this topic to be discussed it would sidetrack us from the main issues, and might even introduce unnecessary difficulties in communication. In the course of the discussion, when it appeared that we might become involved in such an issue, the Coordinators were able to steer away from controversial matters when explicit reference might have been made to it.

However, although we knew that members of the conference, with their different backgrounds and training, would be apt to feel differently in regard to the independent practice of psychotherapy by non-medical therapists, we were equally sure that no member of the group would raise objections to the appropriate utilization of psychological therapists within the medical framework. The explicit hope of this conference was that we would be able to point up areas in which psychologically trained non-medical therapists would have something to contribute to the prob-

lems of the chronically ill, in collaboration with their medical colleagues.

On behalf of the psychological members of the Conference it might, perhaps, be stated that the last thing any responsible psychologist wishes to advocate is the practising by any psychologist of something for which he is not adequately trained. I, for one, would go further than many psychologists and would agree completely with Dr. Kubie and with Dr. Ham in their respective statements that no one, medical or non-medical, should attempt psychotherapy who has not had the opportunity of recognizing his own "blind spots" through some sort of a personal analysis.

* * * * *

At one point in our discussion, considering the factors involved in teamwork, one of our conferees wrote on the board the following list: (1) good will; (2) mutual respect and understanding between all members of the team; (3) clear delineation of responsibility; (4) mutually agreed upon policies and procedures; (5) a joint effort in research training and program application. It might be said that points 3, 4, and 5 came up in some way or other for explicit consideration by members of the Conference as part of their assignment as members of the group. But factors 1 and 2, good will and mutual respect and understanding between all members of the team, are not things that can be created at will, nor dissected out and laid bare on a conference table. Rather the Conference itself carried the responsibility of evoking these feelings in the members of the various professional groups who attended. It is with a feeling of group participation and of real teamwork by all involved that the editor has prepared these proceedings for publication.

CHAPTER I

CARE AND TREATMENT OF THE CHRONICALLY ILL

WHO ARE THE CHRONICALLY ILL?

George Packer Berry

Before thinking about how teamwork can be effectively organized to attempt the problems of the chronically ill, it seems to me the first question is, "*Who are the chronically ill?*" In one sense, all of us over the age of forty or even younger, depending upon our definitions of "chronicity" and "illness." Certainly by the age of sixty all of us have some disorder or other of the circulation, of the bowels, of the bones and joints, or of the nervous system.

How big is the group to consider? Again the answer depends on just what the focus is. If we are thinking primarily about persons who are substantially *disabled* by chronic disease or other impairment, there are involved in the United States an estimated 5,300,000 persons. At least four or five times as many people suffer from minor or non-disabling chronic diseases or impairments. For this larger group, there are fortunately available in most communities adequate services for short-term care. This is not equally so for the disabled group which, I take it, is our major focus.

Where are our chronic patients to be found? Are we to think of them only as patients in institutions such as hospitals, nursing homes, mental hospitals, et cetera? It is well to remember that the cure of disease per patient is probably no greater in the institutionalized group than in free-wheeling individuals. This is true because it is only rarely the case that the disease itself commands institutionalization. There are many other factors outside of the disease that make a human being chronically ill wherever he may be living. Some of these factors are:

A. Mental unfitness—not usually the lack of formal education but the lack of a disciplined mind, the loss of curiosity, of drive, bad habits deriving from years of not studying and not trying.

B. Physical unfitness—often the result of not using the body as much as is allowable—and much more use is allowable than practitioners or teachers of internal medicine realize.

C. Social unfitness—lack of family or friends, or bad ones, or bossy ones.

D. Occupational unfitness—making one an object of charity with the resulting loss of independence, of a job, or of other means of expressing oneself adequately as a human being participating in his social setting.

E. Nutritional unfitness—not just poor diet but poor habits of eating.

You do not cure chronic disease, but you can do a lot for the person who has it. It seems to me that this should be one focus of emphasis in our discussions. In such rehabilitation, about which there will inevitably be much talk, it must be remembered to reach beyond the physical and psychological—it must include social, economic, occupational, and other related areas if it is to be effective. In this connection, the *participation of the patient* is a matter of prime concern. His education must concern every area of his need—physical, mental, social, occupational. To provide an educational opportunity for him a closely knit team drawn from many disciplines will often be required. How to achieve effective integration among the members of the team and to achieve working conditions in which most improvement can be shown while the patient's needs are being simultaneously met, is one of the challenges of the present confernce.

Being a teacher of medical students and a Dean of a medical school, I suppose it is inevitable that I should see closely parallel problems in meeting the needs of the student and the needs of the patient. Therefore, I should like to invite certain considerations to your attention by means of what I regard as parallel situations. Consider the verbs, "rehabilitate" and "educate." It distresses me that these verbs are so frequently used in a transitive way—I believe that they should be almost always used as intransitive verbs.

What does the good teacher do? He works constantly to create the atmosphere—environment—stimulus—help—in which the student can find the best opportunity to achieve his own education. The teacher cannot give an education to a student, much

less drive it into him. So with rehabilitation. The physician must create the opportunity—that implies a great deal—for the patient to accomplish his own rehabilitation.

Rearranging the curricula is almost a national sport these days in medical schools. It is my conviction, however, that for the most part we achieve not fusion or integration, which is what we are after, but merely correlation, co-ordination, juxtaposition. This is all too frequently the case when experts in various of the medical and health fields join forces with psychologists, sociologists, and social anthropologists. How is integration and fusion to be accomplished? Far be it from me to dissertate in the presence of experts in a field about which I learn daily how little I know or understand. I shall nonetheless try to indicate where I believe the central point of the problem lies.

Simple statements sometimes, through oversimplification, lose a point. Let me try one such sentence on you. *Physicians know their patients; specialists know their diseases.* It is a constant job of the individual who wants to be the best physician to accomplish in his own development a suitable balance between the two. It is the job of a faculty of medicine to provide a balanced opportunity for the medical student. Similarly, the teacher teaches students; the "specialist" teacher (biochemist, physiologist, microbiologist, to pick a few at random) teaches his subject—biochemistry, physiology, microbiology. The teacher must achieve the same kind of a balance that the physician must achieve. To help him to do so is one of the most effective roles that the Dean can play.

In the teacher-student relation, as in the doctor-patient relation, the student and the patient must become active partners in the enterprise if educational or therapeutic processes are to flourish.

THE DOCTOR-PATIENT RELATIONSHIP IN CHRONIC ILLNESS*

THOMAS MCP. BROWN

I happen to be one, perhaps now in the minority, in an era where the team approach appears essential, who still believes

* See comment in Introduction regarding order of presentation of discussion.

strongly in the individual doctor-patient relationship, and that the internist who is broadly trained at a high level and who also has proper psychiatric and psychological orientation remains in the sole responsible position. My remarks are expressed in an attempt to balance any tendency toward over-enthusiasm in unilateral thinking which may have been expressed in what has been said up to this time!

When he meets serious psychiatric or other deep problems in this field, the internist should know enough, by virtue of his extensive training, to call for psychiatric or other specialized assistance, which one with less training may not do. The problems of chronic disease are by far the most difficult we have to face and they require the best and broadest type of medical knowledge possible in order to effect improvement. For example, the problems of brucellosis, amebiasis, arthritis, hypertension, diabetes, colitis, Bright's disease and many others are very complex, both from the diagnostic and therapeutic point of view. These conditions present both medical and psychological components and they require highly trained physicians in an atmosphere of medico-psychological integration to effect successful management. Pure psychiatric management without concommitant medical management is ineffective as a long-term aid for these patients. It is frequently stated that the physician does not have the time to handle psychological manifestations, even if qualified. The time he does have is most effective to the patient in providing much needed security with the realization that all of the medical facets are being observed, correlated, understood and managed, not independent but in a manner associated with the psychological needs.

Our real danger lies in our continued emphasis on the two extremes. The general practitioner is so diffuse in his activities and his training that he is limited in his effectiveness in the management of complex chronic-disease problems. The super-specialist, on the other hand, is often equally limited because of his inability to encompass the whole. Only the internist is highly qualified on a broad level and capable of managing the problem of chronic disease. In our institution we have been training doctors as potential internists to meet the enormous growing need in chronic disease. It is my opinion that the internist must and will become the family doctor of the future.

The team has been discussed continuously through this session as a necessary medium for achieving success in the solution of this problem. Some have emphasized the importance of proper team play and leadership within the team and have indicated that the team would fail if individuals concerned did not integrate properly. Any tendency for such conflict to develop is unfortunate and can do harm to the patients. It has been stated that in some teams the person who has the greatest contact with the patient, the nurse in the team referred to by Dr. Davison, for example, or the clinical psychologist in another instance becomes the truly effective patient contact member of the team. This points up the fundamental weakness of the team approach where the all-important patient-physician relationship may be lost. Because medical decisions must be constantly made, the internist should be the closest to the patient and know every aspect of the problem.

The internist of today is far more than the pure diagnostician of several decades ago. He is the one who must detect the first sign of dangerous disease, dispel fear when obscure somatic symptoms do not represent serious disease, and must prevent disease from becoming disabling such as joint or heart abnormalities which require different medical management in different phases of activity and progression. These phase shifts may be very subtle, and they not only require awareness but, most important of all, the knowledge to interpret their significance and to institute proper methods of management.

The major warning which I offer to those who may seek to reduce medicine to independent management of its segmental components is related to the progress of medicine itself. This segmental thinking, if carried to its logical conclusion, will lead to fewer and fewer individuals capable of visualizing the whole. Over the years we have become acutely aware of the tragic consequences to the delicate balance of patients with chronic disease because of conflicting opinions for more than one person in charge of the patient. Progress from research in the complexities of chronic disease is dependent upon total thinking. Where hope of solution of the basic mechanism of a major chronic disease such as arthritis appears most likely to some of us, is to be found in those clinics where the principal investigators are both fundamental scientists and total physicians capable of interpreting delicate

influences in the complex balance observed in chronic disease. Only in this setting would the addition of a clinical psychologist either be wise or helpful. A division of thinking may be more dangerous to the patient than no thinking at all.

My own effort in the field of chronic illness has been principally in the rheumatic diseases and arthritis, and this work has been continued at a fundamental and clinical level since 1937. We have made a point to select and develop individuals in our group who have always maintained a constant interest in all aspects of these diseases at all times. This includes the social, psychological, nutritional, metabolic, microbiological, immunological and therapeutic aspects as they present themselves in each patient. At no time in this research setting has the patient been lost as an individual. All of our investigators, with the exception of a biochemist and a research bacteriologist, are internists first and investigators second. This is a team approach but the members of the team are internists trained to handle the many diverse individual problems as they arise and to integrate the many facets with relation to their relative merit, maintaining, at all times, the patient-physician relationship. In this setting we call upon psychiatrists, psychologists, psychotherapists, orthopedic surgeons and social workers for their help when needed. We also utilize the important contributions of our department of medical integration and rehabilitation which has been very helpful in our objective. Mutual inter-change of ideas and broad therapeutic advantages are always available to the patient and to the group. However, the entire success of this program has been dependent upon the concept that communication of decisions, the basic management of the patient problem, rests in the hands of the single internist in relation to a particular patient.

Arthritic patients may have a host of symptoms which can be interpreted as typically psychosomatic such as fatigue, anxiety, tension, anger, resentment, headache, constipation, poor memory and depression. These symptoms are no more or less important than the anemia, reduced serum albumin, elevated gamma globulin and increased sedimentation rate, or physical and X-ray changes. Consideration and correlation of all these factors is the responsibility of the physician. Without an understanding and evaluation of all these factors in all of the phases of the disease,

one has no true concept of the disease and how to manage it. Of great interest to our entire group has been the correlation we have observed between an effected improvement in the objective hematological findings and the subjective psychosomatic type symptoms when true remission occurs. We believe we have been able to effect a change in the course of rheumatoid disease. This change has been effected through the utilization of a new total concept of the basic mechanism of the disease proposed by our group in 1949. A therapeutic approach has been designed to effect an apparent state of hypersensitivity which we believe to be fundamental in these problems. The most we can say at the moment is that the psychological aspects appear to us to be secondary and not primary and they improve when the disease itself improves basically. Therefore, we must still focus most of our attention upon the basic disease process itself.

Parallel clinical situations which are more definite and clearly defined are amebiasis and brucellosis. Clinical symptoms in these problems have been repeatedly interpreted as purely psychological because of mis-diagnosis of the underlying disease. We have all seen many patients in these groups finally diagnosed properly who have undergone psychiatric therapy for years without basic improvement. Such patients, finally treated medically, may completely lose their entire overt psychosomatic dysfunctions, retaining only their bitterness for the misdiagnosis and the improper categorizations of their problem as primarily psychiatric.

In conclusion, if the team approach is employed in the management of chronic disease, there are certain inherent dangers which, if not guarded against, may not only increase costs but actually increase the severity of the disease through division of authority and opinion. The great danger is to overlook the importance of the broadly-trained, psychologically oriented internist as the key figure in the management of chronic diseases. The internist alone can retain the vital importance of the patient-physician relationship and should, at all times, have the ultimate responsibility of the communication of decisions and the planning of continuous medical management and direction of the individual patient. The internist should integrate the essential role of the physiotherapist, orthopedic surgeon, psychiatrist, the psychologist and the social

worker in their proper relationship to the pattern of overall management. A program which becomes inflexible and routine, or achieves a false sense of security because of the number rather than the individual quality and direction of the many specialists, may serve to promote one of our great present-day medical ills, the loss of the patient-physician relationship. Finally, our greatest single present-day need is a true understanding of the disease mechanism, basic in a host of chronic disorders. This requires a continuous long-term research effort which must be continuously woven into the fabric of the program for management of chronic disease. Research in chronic disease is the key to continuous vitality and ultimate success.

FACTORS IN REHABILITATION NEEDS

WILLIAM T. SANGER

The range of chronic conditions from which people suffer is so considerable that it is not easy to discuss service to these patients without some limitation. For example, whether these conditions are present from birth or are acquired by accident or otherwise may make a difference. If one limits discussion to the larger segment of conditions, it seems to me that there is an alternative approach to the subject before us. Instead of dealing with teamwork on the part of medical and psychological personnel, one can look at the subject from the standpoint of the patient himself. As a point of departure, what are his needs from the standpoint of physical, mental, social, vocational, and educational rehabilitation? Even religion has a contribution to make in many instances.

To secure as complete rehabilitation as possible, the range of needs to be met will, naturally, vary with individuals, but I like to think first of the patient and his needs and then the range of factors required to meet his needs rather than set up any preconceived approach which is not sufficient to meet all of these needs.

Assuming that this general range of services which must be provided is reasonably accurate, the second question is what kind of physical facilities should be made available to accomplish maximum rehabilitation.

Specialized institutions for the care of the chronically ill are, it seems to me, too likely to be detached from the main current of medicine which one finds in general hospitals and in medical schools; therefore, from my own point of view, the ideal location of a faculty for the treatment of chronic conditions is in close association with those institutions responsible for the care of the acutely ill. If thus located, the hospital for chronic conditions can be built at lower unit costs than otherwise because many specialized diagnostic and treatment resources can be found in the hospital or hospitals for the care of acutely ill and relatively short-term patients. Furthermore, if the specialized hospital for chronic illness can also be used for convalescent care, reserving the general hospital for the very acutely ill, it ought to be possible to reduce the cost of medical care for a considerable segment of patients. This may be our next most important step in achieving this desirable goal. Bear in mind, too, that convalescent and chronically ill patients provided for on this basis gives them a maximum in service and is also likely to maintain a higher level of morale in the patient because of the likelihood that he will realize that his special needs are being met, that he is not being shoved away in an isolated institution where, perhaps, he may feel that he is done for.

To begin from scratch on a program of total rehabilitation within the capacities of the patient and the service given him is not a simple undertaking. Many who can make a contribution to the welfare of the patient must first be convinced that the effort is worthwhile and that this effort can best be achieved through dynamic team action. Here one begins to ask himself what is it that motivates a team? If he refers for a moment to the athletic team, he realizes at once that the objective of team play is to win the game. *Rehabilitation is a game directed at meeting total human needs*, and that must ever be kept before those who work on the team. With enthusiastic and competent team leadership which recognizes that every member of the team is an important individual, a good start has already been made. If this team fortunately works in an institution where the whole spirit and atmosphere emphasizes team development and cooperation, the team concerned with rehabilitation finds it much easier to begin its work and to prosecute it with effectiveness.

A PROGRAM OF CARE FOR THE CHRONICALLY ILL

Russell Van Arsdale Lee

There seems to be about five necessary steps if the utilization of clinical psychologists and the cooperation between them, the psychiatrists, and internists is to be made more effective in the management of chronic disease.

I. *Appraisal of the Possibilities.* Before anything else is done it seems necessary that there be formulated a statement on what are really the possibilities in this field. This would have to be both factual and imaginative. Factual in that it would recount what has actually been done in the various aspects of this problem and imaginative in projecting the possibilities if what we presently know is applied and if personnel were available for it. A carefully formulated document setting forth these things would be of great value in enlisting the interest of the public and particularly the medical profession both of whom are to a considerable degree unaware that there is a probably useful field of activity here.

II. *Validation of Results.* Because of the relative ignorance mentioned above and particularly because of the general skepticism that prevails as to the potentialities of the clinical psychologists and psychiatrists in this field, it would seem necessary that there be steadily conducted a careful process of validation of the claims that have been made and that will be made in the future as to the effectiveness of various supposed procedures. There has been a tendency in this field, as of course in many others, for argument by allegation without sufficient controls and without specific measures being taken to validate the results which are alleged to be possible.

III. *Training Program.* It became apparent from our discussion at Galveston and from anybody who is in the field doing this work that there is a great shortage of trained individuals to carry out any extensive program. As a matter of fact most of the good clinical psychologists' time is already completely utilized in acute situations and more or less routine work and if this part of the program is to be expanded, there must be certainly a great many more trained individuals made available. Dr. Kubie's lecture pointed out one of the possible ways that this might be done (See Ch. V). However the length of time involved in this program,

while no doubt necessary, is rather staggering. It would be exceedingly valuable if some method were developed whereby this training program could be considerably shortened without affecting the quality of the product in order to meet the needs which will develop very rapidly if the program of the kind that is visaged is put under way.

IV. *Research.* In addition to the validation procedures mentioned above the first requirement really is for much more extensive research in all fields of chronic disease for possible uses of the kind of collaboration that is being discussed. There should be a coordinated research program formulated possibly under government auspices. Certainly with the collaboration of the various Institutes of Health with a view to the developing procedures that would be useful in the actual field as well as research into the fundamental nature of the psychic processes in order to lay the foundations for sounder programs in the future. The amount of really valid research that ought to be done in this field is almost nothing compared to the very apparent need.

V. *Organization.* Even at the present time with our imperfect knowledge and our deficient personnel the need is apparent for some sort of an organization to make the most of what we have. It is suggested in the previous papers submitted that this might be done by an application of the "regionalization" technique; that there be established in each community a council whose principal interest is the care of chronic disease and this council act as the clearing house for the program in that area, particularly for the allocation of personnel so that they can be most economically used and for the arrangement of proper liaison between the internists, phychiatrists, and the clinical psychologists which is all too frequently lacking unless the individuals concerned happen to be working in a group which has these services already in existence and organized. The very lack of personnel available at the present time requires that they be used as economically possible and this can only be done if there is some conscious deliberate and well-planned organization existing for the purpose.

Summary. It is apparent that in the whole field of chronic disease the personal physician and in this case he would probably be the internist should have the prime responsibility for the care

of the patient but that he should be organized and coordinated into a team with the clinical psychologists and the psychiatrists under such conditions that would make possible constant consultation, collaboration and planning. There is no question but that a great deal of this work will have to be done in institutions and questionably much of it will be under governmental auspices at some level either county, state, or national. Under these circumstances the formation of a team as envisaged above is somewhat easier than if it is entirely in the hands of individual private practitioners. But except in the veterans hospitals where this form of organization has been instituted to a certain degree, it is more or less lacking in the whole picture. The focusing of attention upon the possibility of this collaboration has been the greatest contribution that this Conference has made. I suspect it will have to be followed up my similar regional conferences in order to fully implement the ideas that have been developed at Galveston.

THE ROLE OF NATURAL SURROUNDINGS IN TREATING THE CHRONICALLY ILL

Clarence L. Robbins

I am sure that my only qualification for sitting in this company is that I am an eldering internist practicing in a community that attracts many chronically ill patients. My job is at the distribution-to-consumer level and I daresay I have unwittingly arrogated many facets of therapy. I have done it because my patients have asked me, "What do I do now, Doctor?" and because there was no Dr. Harrower available to give what might be a better answer than I thought I could give. In many instances, I still believe my own answers can be quite adequate to help the patient adapt satisfactorily to his chronic illness. I have in mind the sort of advice a doctor gives to the patient with chronic coronary insufficiency or to the ordinary diabetic patient—and by ordinary I do not exclude the "brittle ones" or the teenaged ones who must be weaned. When my diabetic patient with Kimmelstiel-Wilson's syndrome progresses rapidly toward blindness, then I could fondly wish for a Dr. Harrower. When a patient's basic illness is a personality disorder, I yearn for her.

One factor in therapy of chronically ill patients might be mentioned briefly—simply because I have been privileged to see it in action—more frequently, I suppose, than internists elsewhere. I am not referring to climate-therapy for the peripatetic patients; the heliolatrous visitor is frequently a very friendly fellow. At home he is under the care of our colleagues in scientific medicine to whom we can phone or wire or write with personal salutations. The lines of communication to assure continuous, coordinated medical care are unrestricted—and not unremunerated.

The factor in therapy I do have in mind might be called—transplantation. The majority of the chronically ill persons who pull up the old roots and move to the desert for permanent residence have been and, we can fondly hope, will continue to be self-sufficient citizens who can and will work, who have made provision for their families, who participate in voluntary prepayment plans for hospitalization, who pay their bills and taxes and are the same backbone that supports any American community. The adjustments or adaptations made by these health seekers is a lovely thing to behold. In many instances a complete change of working skills is necessary: The highly-paid brass moulder from Connecticut drives a laundry truck in Arizona but, as he will tell you, "I don't mind too much, Doc, my little girl can breathe out here—and you ought to see my camellias!" The contraction in material prosperity which I daresay is the overwhelming rule for transplanted patients is compensated to a surprising degree. It goes without saying that relief or amelioration of disability symptoms is the Number One compensation. But there is something else, too. I hesitate to call it a spiritual renaissance but it will do for the moment. Maybe it's the vast spaces, or that one can lift one's eyes unto the mountains (indeed you can't lift them,—except unto the mountains), or that there are more stars in a desert sky:—I commend to you Joseph Wood Krutch's *"The Desert Year"* for the best answer I have yet found in writing. Anyway, the chronically ill patient seems to bear his burden of disability more cheerfully; at least, I think so. The Southwest abounds with amateur naturalists, anthropologists and archeologists—the country invites it. As for the painters they are legion and indefatigable; if your hands hurt too much today, hold the brush in your teeth!

Obviously I am describing a certain personality type (I will let my neighbors name it) responding to the experience of chronic disease. I do not know how much of the adaption resides in the established character of the man who would undertake the rigors of transplantation or how much we may thank our stars. But that there appears to be some therapeutic value in transplantation I cannot doubt. Conceivably, we need "a team" to tell us that these happy people are not transplants, they're escapees. All right, tell us but please don't tell them.

Ending on a low plane of economics, I wonder if this group might consider, even in broad outline, a few principles which could be suggested to the State Boards which will presently, under the provisions of Senate Bill 2758, be spending $2,000,000 to survey the unmet needs requiring the expenditure of another $60,000,000 in precisely the field we are discussing at this conference—the case of the patient with chronic disease.

NEGLECTED FACTORS IN THE CARE OF CHRONIC RHEUMATIC ILLNESS

William Charles Kuzell

The true incidence of rheumatic diseases is not known. All older persons show some objective evidence of musculo-skeletal deterioration. Smaller groups have rheumatoid arthritis, gout, and rheumatic fever. To the extent which the full function of the musculo-skeletal apparatus is impaired the individual is deprived from full participation in the socio-economic activity of life around him.

Neglected areas in the management of chronic illness generally and of rheumatic disease in particular are multiple. Not only the "rehabilitation" but the *continued functional maintenance* of the rheumatic patient depends upon teamwork. Too often, of course, the patient himself is the least effective member of the team. Realizing this, it is possible to explore various methods for making the other members of the team bring the patient into full performance.

Prognosis. From the moment of correct diagnosis the problem of education should begin. The tendency of the physician to ex-

plain in one short session the far-reaching implications of serious and perhaps permanent and cyclical physiological derangement to a sick non-medical human being is ludicrous. The failure of proper orientation leads to search for *miracle* drugs by the victim and hope by the physician that the use of these same potions will rid him of the responsibility for long-continued supervision of his handicapped patient.

With the belief that this picture could be improved it is evident that proper prognosis depends upon proper orientation of the physician himself. The mere fact that almost all non-medical people know of some "arthritis cure" is an indication at once of the inefficacy of remedies available in bottles and packages and on the other hand of lack of etiological understanding. While "cure" of rheumatic illnesses is not available, prevention of deformity is possible and presents, generally speaking, the greatest hope for all concerned. This, of course, includes also the prevention of development of mental attitudes by the patient which will prevent the utilization of his limited musculo-skeletal apparatus. Possible methods of improvement include considerations directed both to the physician and to the patient:

Physician Education. (1) Greater emphasis on chronic illness in undergraduate medical teaching. Preparation of *complete case studies* by the student to include the usual medical history, laboratory and radiological reports, and to include, in addition, an evaluation of the patient's home environment, his work situation, and psychological testing of the patient and possibly of several of his immediate associates. (2) Re-orientation of medical faculty members with regard to *rehabilitation and continued functional maintenance* of the chronically ill. This should include proper placement of diagnostic and chemotherapeutic procedures in the framework of the entire problem. (3) Attempted re-education of the practicing physician. This may be implemented by availability of post-graduate "refresher" courses, panel discussions at hospital staff-meetings and medical societies, and by distribution of educational literature. Indirectly some gain may be achieved by the use of skillfully prepared articles in popular prints to incite the chronically ill to demand better and more comprehensive management of their problems by the physician.

Patient Training (To include his immediate associates). (1) Group conferences on various phases of rheumatic disease will permit greater understanding by the patient and his relatives and/or immediate associates who are concerned with his highest functional capacity. Conferences may be sponsored by the Arthritis and Rheumatism Foundation, clinics, and by individual physicians. Motion pictures, possibly prepared by the Arthritis and Rheumatism Foundation, may be made available on 8 as well as 16 mm. film for use by individual patients, physicians, or groups. (2) Schools or camps for arthritics similar to those conducted by groups interested in diabetes. (3) Circulation of pamphlets designed to help the individual and his associates to understand the problem and to cooperate with the physician. (4) Exhibits of labor-saving devices and evaluation of methods of treatment should be available in clinics and possibly in public schools for study by those interested. (5) Incorporation of the idea of proper management of chronic illness in hygiene classes in the public schools.

Nurse Education. (1) Re-orientation of instructors in nursing schools to the end that graduate nurses will show more interest and effectiveness in the care of the chronically sick. (2) "Refresher" courses for graduate nurses (the expense of which will have to be met by foundations and schools). (3) Possible greater emphasis of physiotherapy in nursing education so that nurses may, under proper guidance from the psychiatrist, assist the physiotherapist.

Drug Use. The use of powerful chemical agents to alter the physiology of the rheumatic patient poses additional problems. These drugs are not curative. They may, however, alter profoundly and rapidly the clinical status of the patient regarding physical function. Rapid release from stiffness and pain may encourage the patient to attempt activity which is injurious to acutely involved joints and may promote accidents. Aside from short and long-term toxic side effects of strong medication there is the tendency for both the patient and the physician to de-emphasize physiotherapy, proper diet, psychological re-adjustment, and prevention of physical deformity.

The degree of apparent change in physical status following drug therapy may lead the relatives or associates of the victim into the following difficulties: (a) Acceptance of the concept of

"cure"; (b) the sudden appearance of a "void" in the lives of those who prior to drug therapy devoted much effort and time to the patient, and which may cause acute psychological strains in these third parties, and (c) the development of hostility toward medical people generally upon failure of maintenance of the drug effect.

Employment of the Arthritic. The "rehabilitated arthritic" faces a hostile society. Present employment practices make his gainful employment difficult. The high cost of industrial liability insurance tends to preclude employment of arthritics since the courts and compensation commissions tend to rule that minor accidents either "aggravated" or "caused" the disease. A more enlightened revision of these practices is necessary. There is possibility of arthritics signing waivers and agreement not to sue for aggravation of the disease. The lack of "part-time" work opportunities in current employment practice deserves further study. Encouragement of personnel directors of large corporations to participate in community programs involving chronic illness should be promoted as an educational measure for them.

Diet. Improper eating rather than the lack of food may impair the function of the rheumatic. Although it is generally accepted that for the rheumatic patient (other than gouty) there is no specific dietary regimen to follow, all are agreed that obesity is undesirable. For the proper mental and physical function of the physically handicapped the importance of proper eating is often neglected. Preparation of tasty food in institutions receives too little emphasis. Here again, the lack of sufficient training in nutrition in medical schools may be the basis correcting this problem.

Summary. The number of patients totally incapacitated by rheumatic disease is probably relatively small. The number with rheumatic disease, on the other hand, is probably much larger than generally recognized. The total approach to the problem should not emphasize only rehabilitation but should consider *continued functional maintenance.* The de-emphasis of drug therapy should be encouraged, but this does not condone failure to use chemical agents during appropriate intervals. Prevention of deformity and maintenance of maximum functional status involves elaborate teamwork, both medical and non-medical, and must include the patient.

REPORT OF THE STUDY GROUP ON PROBLEMS OF REHABILITATION, ADMINISTRATION AND RELATION TO NATIONAL AND STATE AGENCIES

Medical and psychological teamwork in the care of the chronically ill requires both a social and physical setting. An important part of the social aspect is the enabling legislation on the national, state, and local levels which provide not only funds, but also directions to the programs which these funds bring into being. The immediacy of the legislative enablement for the construction of some new types of hospital, diagnostic, and rehabilitation facilities seemed to this committee to require the formulation of some broad principles which might serve as a guide to those who are deeply concerned with the most effective care and treatment of and for the chronically ill.

It was agreed that the following principles were worthy of strong endorsement, and that they should be recommended to the total Conference group for discussion and evaluation.

1. The many states have developed a variety of plans for bringing together Federal and Local funds for the construction of hospital facilities. The study group agreed that it would be wise policy for the agency in each state concerned with the administration of the Hill-Burton Act to cooperate with rehabilitation and other commissions to provide approved facilities for programs dealing with medical, psycho-social and vocational coordinations in handling chronic illness.

a. The programs which are to operate within the new physical structures require professional and allied personnel. Therefore, the training of new personnel, the creative utilization of present personnel, and the development of teaching facilities must be part of the building program.

b. The significance of medical and psychological coordination in the research into, and treatment of, the problems of the chronically ill, should be clearly explained and presented to those governmental, voluntary and lay groups who are involved with local legislative action.

2. The original Hill-Burton Act set forth local operational responsibilities, assessment of the extent of hospital resources, and

creation of new facilities within regional plan on the basis of need as the three guide posts. The problems of chronic illness require a planning and coordination which of necessity must extend beyond the hospital walls. This study group agreed that a rehabilitation council could become a central activating and directing core on the local level in the assessment and management of all cases of actual or potential chronic disability. It therefore recommends that the creation of such a rehabilitation council by cooperative action of hospitals and local community representatives be accepted as a sound principle for the development of adequate programs for the chronically ill.

a. It is clear that the realm of function of the rehabilitation council includes the hospital as well as the community.

b. The organization and function of a rehabilitation council, though determined by local personnel and local milieu, shall strive to achieve broader standards of operation and membership.

c. A rehabilitation council should not be the medical-psychosocial diagnostic and treatment team, the employment agency or the hospital administration. It is a center for effective interaction and planning of these various groups so that programs for the chronically ill can be optimally developed within the framework of local action.

d. There are many indications that involvement in the solution of a problem by a group leads to a better appreciation of its complexities, and to a change in values and attitudes. Rehabilitation councils, in their dealing with the problems of the chronically ill may not only lead to changes in attitudes and values among their own membership and in community groups, but as a consequence of such changes may significantly affect the attitudes and values of the chronically-ill patient towards himself.

3. The physical location of a given medical facility for the care and treatment of chronic illness may profoundly affect the type and quality of the medical, psycho-social and vocational rehabilitation programs which are possible. Availability of professional and allied staff, housing, diagnostic consultants, training and research facilities, community vocational and social integrations are only a few of the things which may affect such a program.

The study group agreed to recommend that it is a sound principle to locate new treatment and care facilities for the chroni-

cally ill in close proximity to medical centers so that the programs for this group could profit by the availability of personnel, medical services, training and research opportunities, and general social milieu of the community which is more apt to exist in the environs of such medical centers.

A major area considered by the committee was that of the educational and research contributions of national and regional health associations to the program of rehabilitation of the chronically ill. Limitations of time restricted discussion to one aspect of this voluntary social effort—namely, the education of the patient and his family about the medical and psychological characteristics of his particular illness.

The study group agreed, on the basis of already available examples of such contributions, that it is a sound principle in the rehabilitation program for the chronically ill to have national voluntary health associations prepare and distribute both professional and patient manuals dealing with the medical and psychological aspects of chronic illness.

The following topics were introduced but not sufficiently discussed to lead to even tentative conclusions; the need for techniques and modes of training for coordinated group effort, the utilization of patient participation both in and out of the hospital on the basis of self-organized groups as part of the rehabilitation program; organization of patients into variously structured groups for treatment, social re-orientation, research and community reintegration of the chronically-ill not only as an economical usage of available trained personnel, but because of other positive factors.

The final discussion of the study group centered about the values and achievements of the present Josiah Macy, Jr. Foundation Conference on Medical and Psychological Teamwork in the Care of the Chronically Ill. It was unanimously agreed by all that the growth in communication, the developed understandings, and the importance of the area warranted a strong recommendation to the total conference for continuation of the work of the conference. The Josiah Macy, Jr. Foundation and The University of Texas Medical Branch are to be highly commended for bringing this conference together. The study group recommends that

the total conference express itself on the desirability and possibility of further conferences in this area.

Sanger, *Chairman;* Wright, *Reporter;* Lee, Robbins, Lhamon, Seidenfeld, Rottersman, and Garrett.

DISCUSSION OF STUDY GROUP REPORT ON REHABILITATION, ADMINISTRATION, AND RELATION TO PUBLIC AND PRIVATE AGENCIES

Sanger: There is need in commenting on this report to amplify the point that many disciplines have been omitted from participation in this conference, —an unfortunate necessity of getting together this broad group of clinicians, psychiatrists, clinical psychologists and medical administrators. Anyone sincerely concerned with chronically sick patients would note omission of consideration of religious aid, or of routine educational opportunities for children who have chronic illnesses or who might not be rehabilitated physically. We do not have much time to consider these important matters in handling chronic illness, but at least it should be noted that they have been mentioned.

Our study group has the feeling that this country is considerably aroused at the grass roots by the increasing extent of chronic illness. Congress passed an appropriation of $62,000,000 for care of chronic illness almost unanimously. Recently in Chicago some 400 people assembled from all over the country at their own expense to hear various reports dealing with chronic illness. Many private health agencies are expanding efforts toward obtaining more funds for research in chronic disease, as cancer, heart disease, tuberculosis, polio, rheumatism, multiple sclerosis, muscular dystrophy, myasthenia gravis, cerebral palsy, and geriatrics generally. These agencies are becoming interested in psychological factors involved in chronic disease, both affecting the sick patients, the professional and technical personnel caring for them, and relatives and friends involved.

Our discussion group did not go into the matter of professional and technical personnel, although we noted its shortage, and its deficiencies in psychological orientation. As a group we did try to limit ourselves to practical suggestions which might be taken up by others for implementation.

Shorr: Would you argue for direct action by legislation for implementation of your group's suggestions?

Sanger: No, we are not concerned with direct action. We are interested in gathering information on medical and psychological team-work in caring for the chronically ill, from which may emerge ideas and programs which various agencies, public and private, medical schools, hospitals, or individuals, might utilize for the benefit of those who are chronically ill, or those who may care for them.

Lee: Several members of the group expressed themselves as determined on an individual basis, to carry these ideas to state or private agencies for implementation.

Fremont-Smith: This must be done with the utmost care, if at all. Let us remember that we are not participating in an action conference. The suggestions embodied in this report would wisely be kept as suggestions, and it is not the intent of anyone here to imply that they constitute recommendations from the study group or from the Conference itself.

Kubie: It is important that both the Macy Foundation and The University of Texas be fully protected against the possibility that the suggestions in this report be construed as being approved or recommended by these or any other organization.

Holsopple: Reference is made in the report to rehabilitation councils. Are any actually in existenee?

Leake: Some may exist, informally at least, as here in our own institution. However, the report suggests the wisdom of organizing such rehabilitation councils for the benefit of the chronically ill.

Garrett: Such rehabilitation councils exist in many places, even to the extent of state commissions.

Kohn: Does the Study Group think it wise to encourage the construction of new facilities for the chronically ill, or to augment the quality of care and the amount of study and research in existing facilities?

Sanger: What exists may be suppplemented by state matching federal funds; definite amounts are segregated for the building of new facilities for the chronically ill, for nursing homes, and for diagnostic and evaluation centers. In my work with the Virginia Advisory Committee on Hill-Burton funds, I have participated in planning for new facilities as well as for the expansion of old. The funds involved can only be used for public institutions.

Kohn: Down to the level of municipal agencies?

Sanger: Yes, especially for county centers.

Robbins: Does the program include non-profit hospitals?

Sanger: I'm a bit puzzled on that point. I doubt it, since the emphasis has been on publicly controlled facilities. The main trend currently is toward supplementing existing health centers with laboratory facilities, and with some help at providing facilities for country doctors.

Mrs. Wright: The report of the study group suggests that new facilities for caring for the chronically ill be located in large medical or health centers, where technical resources and trained personnel are available. This proposal does not preclude the value of smaller rehabilitation centers where patients, closer to home, can receive periodical check-ups on an out-patient basis and also hospitalization in specified instances. If we agree that adjustment to chronic illness requires, where feasible, continued functioning in economic pursuits, in the family, and in community activities, then treatment ought to be available which does not jeopardize such functioning. Where the treatment facility necessitates geographic relocation of the patient, the problems of employment, holding the family together, integration within the new community may become formidable. To be sure, during the earlier stages of diagnosis and treatment, it may well be that the patient could receive more comprehensive care at the larger medical center, but as the patient "settles

into" the well-defined chronic phase, it would indeed be of major value were there close-to-home services to which the patient could be referred.

Brown: Such centers might be devised for special use of general physicians. Chronic disease is really one of the most difficult of all conditions to handle well, and it requires the sort of long continuing interest and care which the general practitioner is so well prepared to give. Perhaps centers for intermittent hospitalization for certain mild cases of chronic illness could be developed. From these centers, where specialists might direct the effort, the general physicians might take over for home care, with the sort of team-work we are considering.

Huston: It would be wise for this group to become familiar with other discussions and conferences which have been held on problems of caring for the chronically ill. For example, the American Psychiatric Association in collaboration with the United States Public Health Service held a conference in 1951 in Ithaca on Psychiatry in Medical Education. At this conference there was much detailed discussion of the training of the medical student in the care of mental illness. A second conference in 1952, sponsored by the same groups, dealt with graduate training in psychiatry. Psychiatry is primarily responsible for the largest percentage of hospitalized chronically ill persons in the country. In these conferences some of the content of the present conference was covered, though in a different context.

The present conference has as its focus the concept of the team. This focus brings to the foreground the needs and possible contributions of the several professional groups participating. Thus some of the discussion shifts the emphasis away from the patient, who should be the primary object of interest. The care of the chronically ill person may be extremely complex and difficult. Many professions may be required to provide comprehensive care in a given case. Yet a "team" can never assume the final responsibility for the care of an individual sick person. This is the responsibility of an individual doctor.

Berry: There were two conferences, the second was in 1952, held under the joint auspices of the American Psychiatry Association and the Association of American Medical Colleges on chronic illness: one dealt in part with problems of rehabilitation, the other with post-graduate training of physicians in the care of chronic disease, especially mental. I think our group here can add much to the material resulting from those other conferences.

Wright: An important suggestion of the study group is that there be continuation of this conference effort in this present form. The group feels that there is real point in another conference along these lines, without obligating either the Macy Foundation or The University of Texas.

Leake: It was the feeling of the group that there would wisely be a further conference to include representatives of the various professional and technical groups which would be concerned in medical and psychological team-work in caring for the chronically ill. It would be well to have representation in future sessions from such disciplines as nursing, social service, physiotherapy, occupational therapy, dentistry, pastoral guidance, and hospital administration.

Kuzell: Last year in California we had a conference involving representatives from the various national foundations devoted to single diseases or dis-

ease groups. Our purpose was to explore developments in such fields as fund raising, rehabilitation, cooperation for the benefit of the patient, and general aspects of the care of the chronically ill. To the great surprise of everyone there, the meeting was extremely unsuccessful. If there is to be another conference like ours here now, to include problems on a broad basis in which national foundations are interested with respect to their own particular field, it must be understood that these foundations are quite alert to the uses to which their funds are put, and to their special interests in their own fields.

Holbrook: Is there any group in the country particularly oriented to chronic illness in general?

Sanger: In certain respects the National Society of Crippled Children is.

Lhamon: Let's be sure that we keep clear the matters which might be pertinent to future conferences like this present one. How can clinical psychology best be incorporated in the team-work required in the care of the chronically ill, and how can people with chronic disease best be treated in an over-all manner. In the second I think we have a worthwhile continuation program.

Sanger: With reference to the point previously raised relative to the possibility of those who are chronically ill or have a long-standing physical handicap attempting to compensate for these conditions, I should like to add a personal note. Most of my life I have carried the handicap of conical cornea. High-grade myopia is admittedly a nuisance both socially and vocationally. It takes a good many years to get over being sensitive to visual difficulties but I believe it possible to compensate for them in a variety of ways. One makes a mistake socially and identifies an individual at a party wrongly. He can learn to get out of the possible social embarrassment by saying "Well, what can you expect from a blind man, anyhow?" or something equivalent. Vocationally, I moved from research and teaching to administrative work partly because it was impossible to use my eyes for the former as many hours a day as was necessary. In administration you must give a good deal of time to people and various kinds of planning and program making which do not tax ones eyes too much except occasionally. I soon found that whatever aptitude I may have had for research could be capitalized in institutional development where there is never too much imagination and many of the traits thought to be essential in investigative work. Furthermore, one can become just as fond of administration as he formerly was of his first academic love. I would almost venture the conclusion that more research potentials in administration would considerably strengthen this activity.

CHAPTER II

PSYCHOLOGICAL PROBLEMS OF THE CHRONICALLY ILL

THE EMERGENCE OF PSYCHOLOGICAL PROBLEMS IN PATIENTS REQUIRING PROLONGED HOSPITALIZATION

EPHRAIM SHORR

I have been struck at Macy Conferences with how often many participants have started out with a feeling of discomfort, a feeling which arises from their uncertainty of their ability to make any effective contribution because their specific field of interest seemed remote from the main purpose of the Conference. This situation inevitably arises from the philosophy of the Macy Conference technique as evolved by Dr. Fremont-Smith, for this emphasizes the value and, indeed, the necessity of a multidisciplined approach and, hence, the inclusion of those with scientific disciplines which may not as yet have been directly applied to the topic under discussion. What generally happens in the course of the conference, however, is the confirmation of the wisdom of this philosophy by the introduction of new view points from these fringe disciplines; and these participants, in turn, are often simulated to embark on an exploration of a field from which they would otherwise have remained remote.

Had I not been reassured from having observed this process taking place, I, too, would have felt that it would have been more profitable to give my place in this Conference group to someone more directly concerned with the problems of psychological and medical team work in the care of the chronically ill. I am an internist working in the field of metabolism and endocrine function and am chiefly concerned with making quantitative measurements of physiological and metabolic derangements. Although our patients are often chronically ill, the chief emphasis in their management is on the estimation and the control of the metabolic

and endocrine derangements from which they are suffering. However, one would have to be peculiarly obtuse to remain unaware of the profound importance of dealing with the psychological and emotional problems which arise in patients with these somatic difficulties. Nor would one have to be particularly perceptive to appreciate the advantages for their care which might be provided by the broader approach visualized by this group.

The special conditions under which my research metabolism unit operates have served to call acutely to my attention the psychological and emotional problems of patients suffering from metabolic and endocrine disturbances, not that these are peculiar to this group but rather that they are almost always present. For these studies we enjoy the facilities of a small metabolism unit accommodating seven patients, with a separate nursing, dietetic and medical staff. The physical arrangements are such that it is a self contained unit even with respect to the laboratories in which the chemical determinations are done, which are just across the corridor from the metabolism ward as are the offices of the medical research staff engaged in this study. Another special circumstance is that our studies are of such a character as to require prolonged hospitalization, usually for as long as ten months for each patient, and with some patients requiring a repetition for three or four consecutive years. The pattern of our studies also introduces special problems since most of them have, as their objective, the striking of a quantitative balance sheet with respect to certain components of intermediary metabolism. This means a fixed food intake, constant with respect to each dietary constituent, and necessitates the collection of the total excretions. There are, in addition, frequent blood examinations which require venopunctures, as well as other complicated test procedures involving varying degrees of discomfort with the anticipation of equal discomfort on their repetition. The diets are inevitably monotonous since each patient is usually limited to about three different daily menus which are repeated in sequence throughout the study. It is a further condition of studies of this character that the patient must take every last morsel of every day's diet regardless of how she may feel or what her mood may be. I say "she" because these diseases occur more frequently in women.

It did not take us long to realize that a high degree of motivation on the part of the patient was necessary for these con-

ditions to be fulfilled. Our patients came from every walk in life, from every economic and cultural background and varied in age from 14 to 75. They had a wide variety of disabilities. Some were crippling, and some were merely uncomfortable from the physical standpoint. In our post paralytic poliomyelitis group, they were calamitous, and in the case of our dwarfed children, they were disastrous from the psychological and emotional standpoint of how to fit them into our society. We soon learned that it was not enough to assure these patients that we were aiming at their physical rehabilitation, and that in order to obtain their cooperation in these tedious studies, it was necessary to instill in them an appropriate motivation. To some we could promise relief of varying degrees, to others, a correction of their difficulties; but in almost no cases would these effects be so dramatic and rapid as to serve per se as an adequate motivation. Here then was a microcosm which incorporated all the various problems which challenge any group concerned with the chronically ill.

It was an enriching experience to see how the difficulties inherent in this research program were overcome and to come to understand the many factors that went into their resolution. Each component of this microcosm made its special contribution; and I will now try to describe what these contributions were. In the first place, the stability and continuity of our nursing staff was found to be essential. Ordinarily, nurses in hospitals taking care of the acutely ill see patients for a short period of time. They are chiefly responsible for nursing care of the patient rather than for their personal needs. If a patient should prove personally troublesome, there is no great need to do anything specific about these extra-somatic difficulties except to work out a modus vivendi until the patient is discharged. This could not be the case in our own ward. The problems of the personal adjustment of the patient could not be slurred over or evaded; they had to be met by our nursing staff just as they have to be met within the family and social group. Our nurses and dietitians soon learned how important it was to make the patient feel that she was being respected as a person and not as an object of research and I want to pay my respects to their willingness to undertake a major share in setting up this relationship. They learned how important it was for the eating habits of the patients to be sympathetically considered in the selection of their diets and how helpful it was to

see that the food was palatably prepared and attractively served. They had to learn how to cope with the response to the monotony of the diets and how to serve them at special intervals to encourage their consumption in full; in short, to deal with all of the factors involved in the attitude of the patient towards his dietary regimen. They took advantage of the visits of the patients' families to learn something about their background, their usual activities and aspirations and fears and what they felt deprived of during their hospital stay and to make appropriate substitutions. One result of this was that our nurses got into the habit of taking patients out on shopping trips in their off hours and going with them to the movies. Our occupational therapists were challenged to provide not only diversional activities but also to help the patient acquire skills which might be useful when the hospital stay was over.

Our medical staff which had also been accustomed to brief hospital admissions, now had to learn to assume a broader relationship to their patients and soon began to recognize the desirability of serving as substitutes for important outside relationships—for the employer, the father or mother. They had to, from the onset of the patient's stay, anticipate the degree of permanent disability and to assist in a progressive and sound adjustment to it. This required learning how best to communicate with each patient as to the nature of the disease and what might be expected from treatment. On them fell primarily the decision of guiding the patients' activities in the hospital in such a way that their intellectual interests were satisfied and also to prepare them for suitable activities when they returned to their home environment. We arranged for correspondence courses of various types including stenography and typewriting and provided typewriters for those wishing to acquire these skills. For those of school age, we arranged with the school authorities for their continued instruction so that they would not fall behind in their classes.

Our laboratory staff across the corridor from the metabolism ward were another great adjunct. The patients could see them working at their chemical analyses, carrying out arduous procedures which they knew were related to their well being. The patients were encouraged to visit the laboratories and to help with minor chores with which they could be safely entrusted. In this

way them came to know how much was being done for them on the research level and how much hard work it entailed; and this made it easy for them to recognize how important it was to do their share in making sure that all the food was eaten and that no excretions were lost. This enlargement of the "family" by the laboratory staff took many forms, such as spending part of their lunch hour in the patients' rooms chatting or helping with cross word puzzles or admiring their various productions or sharing a television performance.

And finally we came to appreciate the significance of the contributions of the patients to one another. Their initial resentment over being ill was gradually attenuated by their recognition that they shared their disabilities with others. Our policy of studying a variety of disease entities in one unit was found to have an unsuspected advantage which stemmed from the inclusion in one group of different types of disabilities. A patient with one defect came to feel that she might well regard herself fortunate not to have been struck down with the disability characteristics of another type of disease; thus, the absence of a defect became a positive asset. Then, out of an understanding of what it means to be less than normal, came the desire to be mutually helpful and, with it, an admiration for the courage of others in coping with their disabilities. The father of one of our patients, the latter a fourteen year old girl of abnormally short stature who was under study for ten months while receiving growth hormone, told me that her stay in our metabolism unit was, in his opinion, the most important event in her personal development. A very appreciable contribution is made by our former ward patients who continue under observation in our out-patient clinic, which is located on the same floor. During their out-patient visits, they not only meet those with whom they had been previously hospitalized and cement the bonds which had grown up between them, but they never fail to visit our metabolism ward. The fact that they had returned to their homes improved in varying degrees and sometimes entirely cured is an enormous encouragement to those under hospital study for similar conditions. Firm friendships have grown up and it is very evident that a sense of belonging to a group and to an organization deeply interested in their welfare has enriched their lives.

These are some of the lessons learned by an internist primarily interested in somatic expressions of disease. It is true that the disease group described is a limited one and quantitatively insignificant compared to the more extensive problems of chronic disease with which most of this group are concerned. Out of this experience, however, can come suggestions which might well be incorporated into any broad program for the care of the chronically ill. I am also hopeful that out of this Conference will come suggestions for the management of certain problems of metabolic and endocrine origin which are not associated with malignant somatic derangements but constitute relatively benign physiological disabilities that are nevertheless highly significant for personal well being and happiness. I refer to the problems presented by those of short stature, by the obese and the excessively lean, by the tall girl, the young woman with hirsutism, the physical deformities resulting from endocrine disturbances, the amenorrheal girl and the infertile woman for whom her inability to have a child had become the prime obsession of her life. Their number is legion and they present formidable challenges.

It is with this latter group that I have had most direct experience of the helpfulness of clinical psychologists, both in the study and management of their problems. What I have learned in my first-hand experience has been impressive both with respect to diagnosis and management and has made me eager to incorporate the discipline of clinical psychology as a permanent adjunct to our study group. I have been fortunate in having the help of Dr. Molly Harrower in the clarification of the psychological problems presented by a number of endocrinopathies. These have included cases of adrenogenital syndrome, anorexia nervosa and diabetes, to cite just a few examples. What was so impressive to an internist was the quality of the information furnished by the projective techniques, the insight gained from them into the psychodynamics of these patients' problems, the help provided for their subsequent management, and the extraordinary economy of effort inherent in the use of these techniques. This has persuaded me that our next major contribution to the teaching of internal medicine will come from the incorporation of clinical psychology in the training of the internist. This contribution will not be limited, I believe, to making us better able to manage the many problems arising out of chronic illness, but will also, on the

research level, throw light on the relationship of endocrine function to personality patterns.

Having described these steps in a Pilgrim's Progress to an awareness of the importance of medical and psychological teamwork in the management of the chronically ill, I have, in the process, rid myself of whatever discomfort I may have felt over my inclusion in this Conference group.

CHRONIC ILLNESS AS A PROBLEM IN ADAPTATION TO LIFE

MORTON A. SEIDENFELD

When chronicity becomes a factor, ill health is of necessity seen from a somewhat unique psychological point of view. Ordinarily disease represents a rather abrupt change in the individual's life, accompanied by increased stress and tension and by many frustrations attendant upon altered function. We know amazingly little about the psychological effect of disease when it is persistent for months and years. We are not able to sort out readily and differentiate alterations in behavior due to the long-term altered function from alterations that are part of the regular pattern of the individual. This has been emphasized increasingly in the all-too-numerous studies which have endeavored to establish certain behavioral patterns in specific diseases as due to the disease process itself. Thus we have had such concepts as "spes pthisica," the polio personality, the gastric ulcer personality and the like brought to our attention. While it is entirely likely that certain personality patterns may be linked to certain diseases, we must look much more critically at any explanation that would seek to establish a direct link between the two. It would appear that there is fully as good a chance, if not even a better one, that personality patterns associated with disease may be much more specifically related to environmental factors that influence the patient's life rather than resulting from the pathological process. Even granting that in the psychosomatic and somato-psychic syndromes a close bond may exist between mental and physical factors, we are still considerably short of having demonstrated that the psychic chicken precedes the somatic egg or vice versa. Under any circumstances current investigations which seek to establish

the psychic elements in organic disease are entirely too often carried out without adequate application of the necessity for establishing a baseline of pre-illness behavior sufficient in breadth and depth to warrant the assumption that the altered patterns are actually due to illness. In most of the studies that have been reviewed by the writer there have been few instances when the research has been sufficiently well controlled to warrant the assertions that were made. In every instance one could have explained the findings on the basis of the patient's response to environmental factors such as prolonged hospitalization with associated regression, over-indulgence or rejection of the patient by loved ones, difficulty in procuring employment, the effect of self-prejudice and self-bias patterns toward disability upon the patient, and many other factors without recourse to the pathological source whatever.

In a recent article Harrower has also emphasized the same point of view: "We also found that in the specific environment of the Kaiser-Kabat Institute, where multiple sclerosis patients were encouraged to be as independent as possible, the so-called dependency scores were higher among those patients examined immediately on arrival than in those who had been exposed to the "treatment." The former also showed quite a shift towards extrovertial living as reflected in the Rorschach Test. As a result of these findings, we were left with a feeling that the psychological characteristics could not be considered as predisposing the person to the disease. *Rather that the disease imposed certain standard conditions on the individual patient, leaving its mark to a greater or lesser degree, depending on the patient's pre-morbid personality structure.* When living conditions were changed, changes were reflected in the test score."

It is because of this marked limitation in our ability to assess the impact of chronic illness upon the patient that it appears so very vital that much more attention and effort be devoted to the development of techniques for evaluating the capacity of the patient to make adaptations in the face of prolonged ill health and attendant disability. It is amazing that with all the know-how

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that has been attained in modern medical science we have not yet found an adequate way to appraise the work-capacity of a patient. We continue to talk about "light work" or "moderate work" without truly having the faintest idea of what these terms mean when translated into the capacity of any given individual. Just think about it for a moment. Is light work to be conceived of as lifting sponges or is it, as some think, merely sedentary work. By this time we should be aware of the fact that many a patient can lie flat on his back, not moving a muscle, and yet give evidence of as much labor as he might if he were sitting up in his bed engaged in some physical activity which did not tax him as much as the labor of loneliness, fear and insecurity that is his when he does nothing. It is nothing short of inconceivable that we could not make a great deal more progress in developing methods of getting at the patient's tolerance for emotional, physical and intellectual activity if we would give this problem the attention it merits.

We need far more information about such matters as motivation, level of aspiration, tolerance for frustration and many other psycho-physical correlates which must undoubtedly play highly important roles in influencing the patient's reaction to all illness, but especially to that which is of long duration. The physician, the psychiatrist and the psychologist should work considerably closer with the physiologist and the physiological psychologist to the end that such a point operation might be productive of more accurate methods of evaluating the actual potential of the patient to deal with his environment and with himself.

One final problem in this area has been the source of some disturbance, at least to this speaker if not to others. That is the rather heavy emphasis which is now and has in recent years developed with regard to viewing everything that came within the treatment environment as treatment. Thus, if a patient listens to music he is very likely to be labeled as the recipient of music therapy, without regard for whether the music is doing him the slightest bit of good or not. If the patient reads a book he is the recipient of bibliotherapy and if someone tells him he is looking better than he did yesterday this is sure to be construed as some form of psychotherapy. While this may be a slight exaggeration, it is not too far from what seems to be current practice in a lot of

places. The damage that comes out of all this is the over-emphasis it places in the patient's mind upon the fact that everything which formerly was looked upon as just ordinary activity and ordinary human relationships are now distorted so that he looks upon every event in his life as "not a treat but a treatment." This can lead only to over-emphasis of the abnormal in his life as against the patient's very real need to get back to normality as soon as possible. There is a second harmful effect from the standpoint of the professional workers involved in that the result of this careless use of the concept of therapy has tended to cheapen the real meaning of the word which has in accurate semantics always carried with it the idea that in "therapy" the process is controlled, designed to meet the specific needs of the recipient. Today, in many of these so-called "therapies," there is no basic by which one can determine "dosage," no evidence that there is appreciation of human variability in responding to them, and no indication that the purveyors of the "therapeutic" boons have a basis for coordinating their "therapy" with any of the other "therapies" that may at the same time be going on with the patient.

It is not surprising that there should be failures in the teamwork relationship when so little is done even to establish the philosophy or the basis for many of the procedures used so that all the members of the treatment staff know what is going on. This is not a criticism of the values that may lie in many of these procedures but it is a criticism of the more or less hit or miss basis by which these treatment programs are involved and the failure of those who should know better to recognize that a lot of things can be done to and for the patient in the name of just ordinary human relations without the need to make a "big deal" out of them. If we took this attitude perhaps much better relationships could be worked out by those who really have a definitive therapeutic role to play.

CHRONIC ILLNESS AND HIDDEN NEUROTIC DIFFICULTIES

LAURENCE S. KUBIE

The relationships between chronic organic illness and hidden neurotic difficulties are subtle and complex. This is what makes

the problem difficult from a scientific point of view; and this is what makes the problem of training for work in this field highly complex. To many such patients palliative help can be given by public health provisions and social service aids and by the more intelligent organization of medical care which will reduce to a minimum the secondary neurotic advantages of clinging to the disability; but the core of the problem remains as a challenging scientific issue which requires mature and highly skilled investigation.

Let me illustrate a few of these complications which one encounters most frequently: (a) There are those whose chronic disablement serves as a perfect mask behind which to hide all previous emotional conflicts, conscious, and/or unconscious. If their organic ailment is healed, they are precipitated at once into acute anxiety, depression, or even psychosis or suicide. (b) In contrast to the above there are others whose organic ailments reactivate dormant neurotic difficulties. (c) There are those whose neurotic symptoms change from one syndrome to another, as their organic condition fluctuates in severity.

The incidence of all of these problems is increasing, as a result of the transition through which medicine is passing, in which we can keep people alive but cannot keep them well.

This conference betokens a new sense of responsibility for the care of the chronic patient. But in this new relationship, impossible demands are often made from both sides. The patient demands "miracles" of the therapeutic set-up, often without knowing the nature of the miracle he is seeking because his needs are not only conscious but, also, preconscious and unconscious. Frequently, the same is true of the therapist (whether internist, psychiatrist, physiotherapist, nurse or psychologist).

Out of the frustration of these needs arise many counter-currents of feeling, anxiety, anger, leading to a perpetuation of symptoms and disability, et cetera; while at times a savage drive under the banner of "rehabilitation" there is an effort to make the patient attempt the impossible.

Similar but even more highly charged cross-currents arise within the families and among those who care for the chronically disabled members of a family. Hence the sudden acute collapse into depression or other neurotic illnesses among those who have

faithfully and unsparingly and successfully nursed a relative through a long illness and back to health. The death wishes which were masked behind devoted care while the patient was ill erupt into illness at his recovery. Add to all of these complications the play of unconscious omnipotent fantasies, the expiation of unconscious guilt, and the attainment of unconscious wishes for miraculous bodily transformation. All of these play a role among the participants in the drama, often making of the situation a potentially iatrogenic symbiosis.

When we consider the training and composition of a therapeutic team the question we must ask ourselves is how much deep understanding of these psychological forces must the various component levels of such a team acquire? This is not a rhetorical question. It goes to the heart of any training program.

The "acceptance" of a mutilation or deformity by a patient depends upon his pre-morbid attitude to his own body-image and, indeed, to his own sex. Physical handicaps have multiple idiosyncratic meanings to each individual on conscious, preconscious and unconscious levels.

The socio-economic meanings may be relatively constant and predictable within any culture; but their effects on each individual must vary not only with variable cultural attitudes but with the varied constellation of unconscious attitudes of each patient. This is why one patient will actually welcome marked disablement and mutilation, while another will completely be undone even by a minute disfigurement.

DEVALUATION IN CHRONIC ILLNESS

Beatrice A. Wright

My remarks are pointed to an area which has not as yet been discussed, namely, the *status position* of an ill or disabled person. This position has enormous psychological implications both for the patient and those around him. In our culture, the chronically ill person is often (though not always) judged to be inferior, i.e., of lower status position. In some way he is looked down upon and pitied, because dependency and helplessness, among other things, are devaluating personal characteristics. So powerful is the force

towards devaluation, that even when an individual has commanded considerable prestige, illness and disability threaten his status, *e.g.* a physician, once he becomes a patient, suffers the pain of inferiority.

Not always is the devaluation on the conscious level, for awareness of such attitudes would do violence to ethical precepts. Sometimes even, the relatively healthy person achieves a curious feeling of scurity, for in devaluating another he places himself in a superior position, albeit a temporary one. Unconscious devaluation is also evidenced when a person has the thought, however fleeting, "I am glad I am not like you," for such a notion, interestingly, does not occur when we respect the other person, even though we would not choose to change places with him. Devaluation, conscious or unconscious, expresses itself in multifarious ways. Let us examine a few of these which have immediate significance for the therapist-patient relationship.

Why is it that a patient in a hospital bed is so often treated like an object—unfeeling, uncomprehending, uninvolved? Too often, when he is examined at rounds for example, the personnel surround him, move him and survey him, much as is done in appraising a piece of furniture. Ready rationalizations such as expendiency and lack of time really don't hold much water, for when a person is respected as a full human being, he simply cannot be treated like an object. At least he is talked to and is included as a person in the events which transpire, rather than just stared at and talked about and manipulated.

The lower status position of an ill person also accounts in part for the frequently accepted procedure of excluding the patient from major decisions. Often (though again not always) surgery is mapped out, convalescent care is arranged, employment questions decided, etc. all without the patient's knowledge or at least without seriously consulting him. This may be done because of not wishing to bother the patient, but paradoxically, the patient is often bothered just because he wasn't bothered. Then, after everything is nicely worked out for him, we condescendingly elicit his cooperation, much as we demand obedience in our dealings with a child. In fact, the psychological situation of an ill person in some respects is parallel to that of a child, especially in their position as minority groups. Should the patient not follow

through with the treatment plan, one becomes annoyed as one would with a child, or patronizingly scolds him which is but another expression of underlying devaluation. It hardly occurs to us that in some measure the childish behavior of a patient may reflect his childish treatment. Of course, we are not referring to instances where the patient is in no condition to participate actively in decision making.

The problem of the patient who does not follow through the plan of treatment may also have its roots in self-devaluating feelings. Not only may he fear the physical implications of his illness, but he may fear *even more* the stigma of being worthless, a burden, unmanly, etc. A diabetic for example, in spite of "knowing" the rationale of his diet, may flagrantly spurn it, for in so doing he tries to deny his disability and the inferior status it connotes to him.

A practical suggestion emerges from this analysis. There is no apriori reason why selected patients shouldn't be included in conferences concerning their cases. Why should those who are helping keep secrets from those who are being helped? Indeed, in some respects the patient is more expert than the experts. More than that, it enables him to assume the adult role of shared responsibility, a role which can go far in extricating him from the feeling of self-devaluation to which he is also prey. Research on patient participation in the treatment team is needed for a clear formulation of principles which can be used as guides in the selection of promising patients for this purpose.

There is yet another phenomenon which reflects belittling attitudes towards the sick and disabled. In an effort to protect the patient, there is a natural tendency to make light of the illness. Sometimes the true diagnosis is kept from him, or certainty of eventual recovery is held out when none exists with reasonable expectation, etc. Though the intention, on the conscious level, is laudable enough, it is supported by the underpinnings of devaluating attitudes which "feel" in effect, "My dear fellow, you won't be able to take it. I shall have to humor you along . . . ." Actually, more often than not, the prediction that the patient will be catapulted into unrecoverable despair is erroneous and reflects rather the feelings of the judge who is in a relatively healthy state and who therefore does not have to face and to cope

with the problems of disability. Research has already revealed that there is very little relationship between adjustment and the severity of disability. The patient may even suspect the true nature of his illness and be well aware of the attendant difficulties so that light-hearted pretense, in challenging his strength of character, may be interpreted as degrading and only function to threaten his self-image. Besides this, the mourning reaction, as discussed in a later paper, is a necessary part of acceptance of loss and disability, which means that depressed feelings are not always to be side-tracked. And yet further, just as devaluation is associated with helplessness and dependency, the chronically ill person who manages in the face of adversity achieves a *salutary status position*. He has to know what the score is in order to show that he is not defeated by his illness and it is then that he merits an added measure of respect. This, of course, is not denying that there are special instances where protection in the form of shielding is indicated, though we again submit that such instances are far fewer than one intuitively might anticipate.

What seems clear, then, is for all persons—those working with the chronically ill, the chronically ill themselves, and those who will become chronically ill—to face intimately their attitudes and feelings toward disability and illness. As long as underlying devaluating attitudes exist, behavior which devaluates will follow in its wake, even though it might have to cloak itself in the guise of innocuous expediency, shielding, and so on.

COMMENT ON DEVALUATION IN CHRONIC ILLNESS

Russell Meyers

A number of speakers have used the concept of the patient's own devaluation of his disability and have suggested that one of our major tasks as therapists and investigators consists in revising these evaluations for the "betterment" of the patient, his relatives and associates. As a generalization this promises to be highly useful. I wish merely to add that a discussion of "evaluation" at a somewhat different level of abstraction than has engaged our attention thus far may provide the kind of key we are looking for

in the prophylactic realm as well as in implementing research and instituting psychotherapy.

A convenient jumping-off place for this discussion is provided in Dr. Beatrice Wright's report of the diabetic patient who, having been given certain instructions concerning his illness, was presumed by his physicians to have "known" what was in store for him and therefore what was needed to avert further difficulties. Despite this the patient rejected, or at least did not accept and employ, the advice. Dr. Wright properly raised the question, "*Why* did the patient not act in accord with this good advice?"

At this point it would be well for us to recall that "knowing" is a *multiordinal term, i.e.*, it possesses several readily distinguishable meanings. From at least one point of view, that of the physicians, the patient must certainly be said *not* to have "known," for "knowing" in an operational sense can be ascertained only in terms of what the patient *does*, not by recording that to which he gives token assent by lip service, head nodding, empathetic smiling, etc.

We would do well to note that the patient, like the rest of us, can react to any stimulating situation impinging upon him—whether professional advice or the play of sunshine, wind and rain—only by responding to a part as if to the whole. At any given moment, his reaction is in considerable measure determined by the play of a mere *part* of the current situation upon his previously established repertoire pattern (which have been similarly derived from part-as-if-to-the-whole reaction experiences).

This group needs no amplification of the principle that the organism is so constituted that it has no other way to react to its intra-and extra-organic environment than by selective response. Its sensory receptive equipment is limited to relatively narrow ranges of relatively restricted modalities of excitation. From the surface receptors centralward, neural energies alone must act as surrogates for the plenum of physical energy changes of the intra- and extra-organic environment. The latter are transmuted to a common form of physico-chemical events called neural currents, the fundamental features of which, considering the variety of peripheral sense organs aroused, are extraordinarily similar in their ranges of frequency, voltage and wave form. These

neural impulses rapidly enter the busy milieu of already prevailing neuro-humoral activities of the organism where, to the best of our knowledge, "modulators," "filters" and basically simple binary reactions ("open"-"close") in the nature of synaptic responses determine a still further selection of parts of the input. They are *parts of parts* which must stand for the *whole*. Ultimately, a more or less recordable organismic response, again, within a relatively narrow range of possibilities of muscular and glandular activity, appears. Such constitutes a response to *perceived relationships*, not to "physically" specific stimuli.

The inevitability of this reaction-to-a-part must somehow be tied up with the processes of "knowing" and "evaluating." It is to a consideration of this that your attention is invited.

When an organism behaves in a relatively "adaptive" way (speaking in terms of the "negative entropy" used by Schroedinger in his characterization of Life and of "information" as used by Shanon and Weaver) it invariably reacts to a *crucial stimulus* (Sc_1) within a total matrix of stimuli presented to it in a given situation. This selectivity of response is called by the psychologist *discriminative behavior*. And when in future situations that present similar but not identical crucial stimuli (Sc_2, Sc_3, etc.), (Sc_1) the old "model" facilitates the appearance of the already acquired adaptive response, the behavior is called "generalizing." This "generalizing" appears to be as inevitable an organismic process as "discrimination" itself. It may of course have various consequences for the organism, ranging from "good" to "bad."

We now go a stage beyond. If the organism proves capable of substituting (*i.e.*, reaching to) a label or other symbol for his now "generalized response," we say he has "abstracted." The capacity to "abstract" in this sense (epitomized in such invariant vector quantitations as Ohm's $E = \frac{I}{R}$ and Einstein's $E = M_c 2$) also carries graded potentials for "good" and/or "bad" in various contexts. The point of significance for our present discussion, however, is that both generalizations and abstractions tend to become organized as substrates of more or less enduring organismic reactions. Such are technically spoken of as "attitudinal sets," "orientations," "value systems," "models," "maps," etc. Their

distinctive effect is that of being self-reflexive and self-realizing, *i.e.*, they go far in determining the further discriminations of which the organism is capable. All this can be schematized in simple fashion as follows:

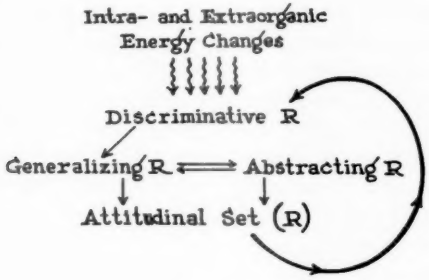

FIGURE I

We are now prepared to grasp the point that unless relatively uncommon circumstances prevail, the repeated play of this sort of action within an organism tends to substitute a *static orientation* for what is actually a *dynamic process-environment.* In brief, the organism's maps tend to be defective representations of his world. The danger-laden influence of continuously applied labels (*e.g.*, John Doe, *the* Doe family, *the* Baptist Church, *the* Democratic Party, and Western culture) is that they endow agencies which are ever-changing with a spurious, static character. This influence must receive consideration in our present attempt to understand how patients can be deterred by their own attitudinal sets from making adaptive R's.

It helps us understand why the diabetic patient fails to follow instructions. More broadly, it helps explain why patients beset with a chronic disability fail to revaluate themselves in a manner envisioned by their well-meaning professional advisers as consistent with the changed circumstances of life.

The next issue, namely, whether all this is organismically *inevitable* and therefore sounds a discouraging note, can be quickly dealt with and, fortunately, somewhat optimistically. For a static orientation is in no sense an inevitable consequence of responding to a part *as if* to the whole. It is merely the result of a coexistent

cultural habit of over-emphasizing likenesses and deemphasizing differences. *This habit can be otherwise.* Various experiments, represented by Oejeman's work with young children at the Iowa Child Welfare Station, spell out the welcome assurance that attitudinal sets can readily be developed which are oriented for *change*, *i.e.*, for a process rather than a static environment. Such appears to pay big dividends. Hence, if there is any one principle of sound clinical hygiene, any promising line of research I might have the temerity to suggest to this august company, its theme is that people can be systematically attitudinized for the inevitability of change within themselves and their environment.

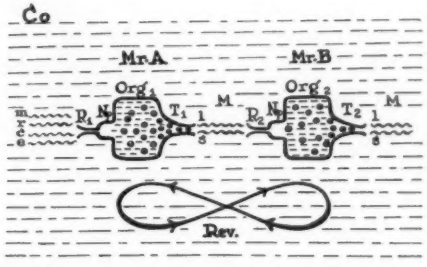

FIGURE II

To crystallize our consideration of the "knowing" process, the attention of the Conference is invited to a diagram, one among several similarly useful models recently proposed, which depicts the communicative process as it occurs between Mr. A and Mr. B (Fig. II). A special case, that in which Mr. A is also Mr. B, can easily be imagined. Indeed, *intra-personal* communication must be thought of as an inevitable concomitant of every conventional instance of *interpersonal* communication. I have here introduced a few approved modifications of a basic diagram devised by my good friend, Wendell Johnson.

This figure is somewhat more detailed than figure I, but is otherwise entirely consistent with it. To elucidate: Mr. A and Mr. B obviously do not and cannot communicate in vacuo. They communicate always within some frame of reference, *i.e.*, within a context, a space-time-physico-cultural background which plays a considerable determining role in respect of those activities of

Messrs. A and B which attract most notice. This contextual background, Co, is schematized as a large open box containing broken straight and wavy lines. The latter represent the "local" world of quantum environmental energy changes, intra-and/or extra-organic which are insensibly continuous with the general world. Now, to some of these energy changes (m = mechanical, r = radiant, c = chemical, e = electrical), Mr. A's receptor apparatus, R_1, may at a particular time and place be relatively oriented. Here occur the first-level discriminations. Mr. A's receiver, R_1, can only propagate impulses centralward through the channels N_p (peripheral neuro-humoral pathways). The latter so transmute the environmental energy changes that in their new form they bear little resemblance to the original. As already noted, a considerable homogenizing is accomplished here, *i.e.*, a part-for-the-whole process once more occurs. The impulses next reach the brain and, more broadly, the chemo-hormonal moiety of the organism, Org. 1. Here a busy dance of molecules and atoms, of electrolytes, colloids, etc., is already in progress. The dance is inevitably altered to some degree by the advent of the newly-arrived bits of information. These neuro-humoral events (quanta, represented in Mr. A's organism by the short broken lines) are the only things Mr. A ever can directly experience, although by a curious process of "projection," he confidently believes he "knows" m, r, c, and e directly.

We should emphasize at this point that all of A's implicit and explicit responses, including those that stand for his evaluative *attitudinal sets*, refer to these intra-organic events and these alone. Now, pertinent to these events and their complex interrelationships Mr. A has a number of *symbols* which attach to only a few among the plenum of his neuro-humoral events. These *symbolized events* are indicated by the broken lines within the white circlets. But during any particular act of communication, Mr. A can command only a restricted number of these, *i.e.*, a selection must once again be exercised. This symbolizing and selecting is in effect an encoding process, carried out by A's transmitter in the exercise of his linguistic habits. By it, Mr. A sends "information" through the medium of transmission, M (air, wire, etc.). The symbols he actually employs are arbitrarily represented as black dots. These, we should note, are not the "same" as the broken lines (neuro-humoral events) but are mere *tokens* that stand for

them. From M they are conventionally picked up by Mr. B's receptor, R_2, usually in the form of "light" (l) and/or "sound" (s). Within Mr. B they are now decoded. Such process is now repeated over and again bidirectionally. The to-and-fro loops at *Rev.* symbolize the reverberating, self-reflexive elements that feed back on Messrs. A and B as communication continues.

But, the patient listener may ask, what has all this to do with "knowing"?

It seems to me to provide a useful model for perceiving that what Dr. A "tells" Patient B cannot possibly be *decoded* in Org. 2 *as it was encoded* in Org. 1. Entropy unavoidably comes into play during every act of communication. Hence, we should not be surprised that Patient B sometimes gives sincere enough verbal or other symbolic "assent" (*e.g.*, gestural, mimetic) to Dr. A, *i.e.*, uses black dots similar to those used by Dr. A, yet shares little of the latter's *preverbal neuro-humoral activities*. This is ample reason why he behaves "inconsistently" (by A's measures). Mr. B's promptings to action are largely determined by the preverbal neuro-humoral events going on within him, not by those of A. They are also determined by his established verbal habits, attitudinal sets, etc., which also differ from those of A. Only where B's preverbal organization corresponds fairly closely to or permits him blindly to accept A's will we observe him accepting A's advice *re* diabetes, or anything else.

In brief, our criteria of B's "knowing" must somehow be concerned with the preverbal events that lead to B's action as compared with A's. Most conveniently such criteria can be identified and quantified in terms of what B actually does, as opposed to his implicit and explicit verbalizations. This is what was alluded to yesterday in my remarks concerning the measures of (a) what one "knows" and (b) what "information" we may reasonably expect to derive from him.

ANXIETY, CHRONIC DISEASE AND THE PSYCHOLOGIST

EUGENE C. MCDANALD, JR.

When I think of chronic disease from the point of view of a psychiatrist, I think of chronic loneliness and anxiety as possible

causes and manifestations. I have pondered the question many times as to what our institutions, social, and educational, et cetera, might do to reduce the anxiety and loneliness that is present in individuals in our society. This Conference sharpens the focus of my wonderings by bringing to the fore what medical practitioners or specialists might do in collaboration with the non-medical psychologist to spot the significant emotional factors that may be the cause or the accompaniment of chronic illness. In this connection a case of chronic anxiety with chronic colitis comes to mind. This patient said to me in the rather hectic course of her therapy, "If I ever have to go home again I'll die." Several months later she was removed from therapy by an aunt upon the authority of her family who had strenuously objected both to her being away from home and to the expense involved. Prior to leaving this area she told me again over the telephone that if she was forced to go home she was certain she would die. I indicated to the aunt who took the responsibility of taking the patient home, that in my estimation, it would be a serious mistake to remove her niece from therapy until she could return and face the home situation of her own accord. The aunt, a person who had had an intestinal resection and whom, I felt, presented evidence of a serious personality disorder, refused to abide by the recommendations made and took her niece home. After the patient had been home for a three-week period she developed an acute intestinal obstruction that was fatal.

This patient had been ill for many years, according to her history. The question that I raised initially was, "Why was it that her serious emotional problems were not picked up at a much earlier period in her life?" My own feeling is that this case is typical of many cases of severe chronic anxiety complicatel by somatic difficulties in which the anxiety could have been spotted earlier. (I intend no unkind criticism of the internist who handled the case for many years, for I agree with the late Dr. A. A. Brill, that everyone does the best he can). In hearing the historical data I could not help but listen with considerable regret to this patient as she related her long experience with medical men without the benefit of a psychological evaluation or psychiatric help.

As to the prevention of such instances of chronic psychosomatic disease, I feel it is important to accent the early spotting of

degrees of anxiety that may have serious psychosomatic repercussions. How? It is here that I feel the clinical psychologist can contribute his skills and in some measure give the psychiatrist and the non-psychiatrist practitioner the help they want. A great many psychiatrists and a few fortunate general practitioners know that a competent psychologist can tell them something about their patient that they do not know. Both know they can get qualitative and quantitative help from the psychologist about the relative importance of psychogenic versus somatic factors in an illness in which they are unable to weigh these important determinants by themselves alone. A case in point is a person whom the doctor, a general practitioner, and the patient's relatives, thought had essentially functional difficulties. Psychological testing showed no special anxiety, but rather a degree of integration commensurate with emotional health. The physician, on the basis of the psychologist's findings, restudied and reworked the case and found chronic amebiasis which when treated resulted in a clearing of all of the symptoms that prior to that time had been pronounced psychogenic.

From personal experience I believe that psychological testing is needed in every department of medicine for patients who have a prolonged convalescence and for those who are chronically ill and do not tend to improve. Such testing is not only sound for diagnostic reasons, but sound also from the standpoint of therapeutic and economic considerations that affect the patient vitally.

So far I have referred only to chronic disease in the patient and the importance of the contribution of the psychologist as a member of the therapeutic team operating in behalf of the patient. Now let me for a moment say something about chronic disease in the doctor, especially as it relates to the psychiatrist and his unhealthy emotional reactions that get in the way of his work with patients. To this end I would like to note that in The University of Texas Psychiatric Residency Training Program we feel that it is important for each resident to carry in intensive psychotherapy a difficult patient, perhaps even a patient that the resident would not ordinarily select. It is our impression, since our residents are not in a position to get analytic help for themselves, that they are able to get at and understand many of their significant anxieties as they are evoked in the therapeutic work with the patient. By

the time the residents complete their training most of them have become aware of a significant fraction of chronic disease which they carry within themselves that is represented by their inappropriate emotional (counter-transference) reactions. To facilitate getting at the counter-transference reactions it is felt that the psychologist, by means of projective tests, can assist the supervising psychiatrist in pointing up the anxieties in the resident that make for "blind spots" (chronic disease) in his interpersonal relationships with patients. To test the validity of this assumption a research project is being established.

ANXIETY IN CHRONIC ILLNESS

ROLLO MAY

I am chiefly interested in the meaning of chronic illness and its special relation to problems of anxiety and responsibility on the part of the patient. I shall speak centrally on the basis of my personal experience of several years' struggle with TB, which occurred in the midst of my psychoanalysis and thus at a time when I was presumably equipped to some extent to try to understand psychologically the experience of being chronically ill. My experience was before the contemporaneous drug and surgical therapies, and hence TB had the psychological impact of a real chronic illness recovery from which, if it occurred at all, was a long struggle at best.

Understanding chronic illness—speaking of TB and similar diseases—requires a different concept of disease from the usual one popular in our society, namely that the disease is due to "fate" or the accident of the patient's having picked up some harmful baccilli. I presume we would all agree that we always consider chronic illnesses like TB as more or less psychosomatic. That is, there is conflict within the person concerning his relation with himself and his world, and therefore greater or lesser anxiety present among the causes of his breaking down with the disease at that particular moment in his life career. The type of anxiety that is involved in the etiology of chronic illness like TB I think is generally of a relatively severe sort. Speaking schematically, conscious anxiety occurs when one's conflicts with one's self and the

world can be handled more or less as they arise. When such anxiety cannot be handled, psychological symptoms generally appear; when the anxiety is more severe and persists over a period of time, the organism may resort to psychosomatic symptoms; and when the anxiety is still more severe and persistent, chronic somatic symptoms may result. I think that chronic illness like TB, from its psychological side, represents a relatively severe form of despair, discouragement and conflict. It is a shrinking of function of the organism because of repressed despair of being able to manage the anxiety any other way.

The disease serves to "bind" the anxiety, to use Freud's word. But as the person begins to overcome the disease, the binding no longer works; and the person's psychological problems begin to emerge. This is widely recognized among TB patients. At Trudeau Sanatorium patients, noting the emergence of the clear psychological problems in themselves and their fellow inmates, would remark, "The sign over the gate should read, 'This is a sanatorium for mental cases with a little TB on the side.' "

This analysis means that the patient's attitude toward the causes of the disease needs to be different from the ordinary one in our society. When well-meaning friends and medical personnel in the sanatorium would reassure us that the disease had occurred because of the accident of our picking up an infection, thinking thus to relieve self-blame, they actually made the more psychologically sensitive patients more discouraged than before. If the disease were an accident, how could one be sure it would not recur again and again?

But if the patient realized that something had been wrong in his previous life pattern and behavior and this had been connected with his coming down with the disease, then he might have felt more guilty and responsible but at the same time he could, and often did, feel more hopeful. For if something had been wrong in his behavior and attitudes, this could be changed; and he could then have some hope of really overcoming the disease.

What underlies the conflict, anxiety and despair will, of course, vary from patient to patient. But the regimen which will help him overcome the disease must at the same time be the one which will help him overcome his original conflict and anxiety. I would say that a person recovers health—or, actually, we should say

achieves health, for it is not enough that he merely be restored to his previous state—only as he overcomes the psychological problem in his relation to himself and his world which was significant in his becoming ill.

In the Sanatorium there were three types of patients with respect to their attitude toward the rules and regimen required to "cure" the TB. One group rebelled against the regimen: they tended to get worse and die. Another group simply accepted the rules and followed them by routine: this group tended not to get worse, but to remain on a static plane. This approach, unfortunately, is I fear the most common approach to illness in our society. It is a negative, defensive attitude. The third group was engaged not only in curing a physical disease but in using the experience as an opportunity for understanding and overcoming problems in their way of life, in their relation to themselves and their world, which had been contributory causes of the illness. These persons, with their positive approach to their illness, are the ones who generally emerged from the illness fuller, healthier persons than before. I mean something here much more specific and profound than simply the platitude that one is a better person because one has suffered. I mean a process like that involved in really overcoming a neurotic symptom: the person has not *just* been relieved of the symptom but has been forced to achieve a higher level of self-realization and integration than he possessed before.

This implies a quite different concept of the patient's responsibility. It also implies that the physicians' job, or whoever works with such patients, is chiefly that of educator. We are using the term "educator," of course, in a much more profound subtle and personal sense than its usual meaning. For unless the patient learns to take care of himself no one, be he physician, psychologist, family or friend, will be able to do it for him.

THE PERIOD OF MOURNING IN CHRONIC ILLNESS

M. Erik Wright

The acute stage of illness usually is accompanied by a sudden and massive constriction of the physical and psychological life

space of the individual. Figuratively and literally this life space shrinks "down to the skin" as the struggle to live overshadows all other considerations. It is only when life is not so immediately threatened that the individual becomes more aware of his changed relationship to himself and his environment. If this awareness brings the realization that a state of chronic illness lies ahead, then the mourning reaction for what has been lost envelopes the individual.

There are several aspects of this mourning reaction which should be considered.

Initially, there is the emotional reaction to the felt "catastrophe" which has overtaken the individual. This may express itself in overt emotional despair or in acutely felt inadequacy, hopelessness, defeat or plain numbness—emotional withdrawal. However, the mourning of the loss does not end with the subsiding of this initial, intense emotional response. The mourning may be perpetuated by one or more of the following factors.

The factor of change, per se, introduces a shift in cognition so that the differences in the new situation are brought into prominent focus. The *change* in the physical status of the patient resulting from the acute illness tends to emphasize what has been lost, the physical capabilities which are no longer present. The capabilities which have not been impaired, just because they have not significantly changed, are unattended to and even forgotten at the peak of mourning. Moreover, preoccupation with the loss leads to a characteristic distortion in which the loss is perceived as so much greater than it may later prove to be. This emphasis on the loss aspect is the core of the mourning reaction. The cognitive structure of the individual's life space which brings about the mourning state, is itself supported by this emotional response.

An important motivational factor is the need to cling to the past. As long as the "psychological past" can be kept actively alive and in the present, the memory of the abilities which were available then can be cherished and the sense of loss can be deferred. The mourning somehow is nourished by and yet itself sustains this wish for the past. The wish to be as before, to maintain the self-concept and the body-image unaltered, accentuates the feeling of loss. The mourning reaction itself engenders a hopelessness

which sees no satisfaction in the present which would make possible a gradual yielding of the past.

And then there is a social aspect of mourning which is closely related to the questions of values and acceptance. Dembo and her associates have pointed out, in their research on the physically handicapped, that the disabled person has the same value which intact persons have about their physical abilities, especially early in his disability. Because of this the value cannot be changed too abruptly or minimized without the threat of estrangement from and by the intact group. Thus there is a kind of "requirement of mourning" which arises from the internalized values of the person who has suffered the illness and also from the expectations of the social group that proper respect through mourning be shown for the loss. It would therefore be therapeutically unsound to attempt to shorten too rapidly, this mourning period. The cheerfully intended "Oh, you're not so badly off. Look at what you can still do," or the artfully contrived methods to divert the patient's attention from himself may only lead to feelings that these other people do not understand, that they do not appreciate his loss. Especially is this apt to be the consequence of the premature reassurances to the patient about the future if the advocate is himself intact in the area of the patient's loss. To give up easily these valued physical abilities implies that they were not important, their loss not significant, and the mourning an over self-indulgence, all quite harsh self-criticisms. There is also the possibility that if the distraction techniques work too well, too early, without any beginnings of change in values, then when distraction is no longer effective, and the full realization of loss returns an exaggeration of the mourning may set in. The loss aspect is not only recurrently dominant, but also the belief that one can adjust is weakened.

These seem to be some of the factors which are associated with the onset and maintenance of the mourning reaction for loss when the patient becomes aware that he has entered a state of chronic ill health.

The physical and psychological necessities and realities of living intrude themselves from the beginning and for many who have become chronically ill, these can be the starting points for the development of new coping mechanisms, for the acquisition

of new motivations and for changes in values which are necessary in the process of overcoming mourning. Just as mourning for loss does not cease abruptly, so also is there a time requirement for the reconstruction of a body-image (if such ever does take place) or at least for the re-evaluation of the self and the re-establishment of a sense of worth. Adjustment to chronic illness means the avoidance or inhibition of previous activities as well as the acquisition of new coping mechanisms. The recognition of the "asset values" remaining to the person can take place as the life space becomes structured with meaningful goals and possible paths.

THE PREVENTION OF THE FOCUSING OF FRUSTRATION IN CHRONIC ILLNESS

CHAUNCEY D. LEAKE

In medical and psychological teamwork in the care of the chronically ill it may become increasingly important to recognize the psycho-dynamic forces originating in the individual emotional drives and needs of the chronically ill patient. This may help both the patient and those who may be involved in the patient's care, to direct the energy of the emotional drives toward a relatively wholesome and reasonable adjustment to the illness. It may also aid the patient and those in attendance on that patient, whether physicians, psychologists, nurses, social workers or other attendants, to be able to understand the various steps in the direction of the drive toward a healthy mental and emotional balance in relation to the chronic illness, or on the other hand to appreciate the steps that may turn the direction of the patient's emotional drives toward increasing anxiety and insecurity with resulting difficulty.

Among one's individual emotional drives and needs generally are: (1) an urge toward aggressive dominance; (2) a longing for submissive security; (3) a need for the satisfaction of physiological demands; (4) a desire to love and be loved, and (5) a drive to obtain an opportunity for self-realization, to reach the "ego-ideal." It may greatly help both chronically ill patients and those who care for them to evaluate the relative pressures of these

emotional factors in each individual case. Testing methods in clinical psychology now permit a tentative approach to the appraisal of the relative strengths of these emotional factors and thus to provide a basis for their discussion and control. The assessment of these basic needs in patients with chronic ills may provide foundation for obtaining a relatively satisfactory adjustment to the chronic condition in individual instances, as has been emphasized by Doctor Seidenfeld in polio, and by Doctor Harrower in multiple sclerosis.

If, as Maslow suggests, confidence can be placed in an individual's own instinctive judgment regarding emotional matters and their development, one might very early promote steps in the direction of orientation of psycho-dynamic forces toward the goal of healthy mental and emotional equilibrium. With the onset of chronic illness, however, a patient naturally feels emotional disturbance, and anxiety and insecurity are certain to develop. The chronically ill get more than their share of "free floating anxiety."

The first step in orienting the psycho-dynamic forces in an individual toward a satisfying personal and social adjustment, especially in chronic illness, is to help obtain self-knowledge, with growing understanding of one's self and one's environment. This effort at self-knowledge, as Doctor Lawrence Kubie insists, should be in depth. In itself, this undertaking would assist one to appreciate the limits of the specific situations existing, and thus prevent the temptation to indulge in hope that may be impossible of fulfillment. With increasing self-knowledge and understanding, the chronically ill patient might be aided in an active effort toward a balanced satisfaction of the emotional drives and needs which are involved, and which can be reached in the face of the circumstances of the particular chronic illness and the environmental conditions involved. Even the occasional achievement of such a satisfaction can reinforce the possibility of attaining a healthy mental and emotional adjustment to the conditions of the chronic illness.

On the other hand, it is equally as important for the individual with chronic illness, and all those who may be involved in the care of that person, to realize the various steps that may go in the opposite direction, and to recognize them as promptly as possible,

in order to try to turn the drive toward the positive healthy balance. It seems that the first step in the path toward an unsatisfactory solution of the situation is the development of symptoms of anxiety and insecurity. These result from the diffuse unconscious fear that the basic emotional needs of the individual *may* not be satisfied. The recognition of the first symptoms of anxiety and insecurity on the part of the chronically ill patient may help at once in laying the foundation for a systematic discussion that may help orient the sick person more satisfactorily toward a reasonable adjustment. Of course everyone, on first realizing that a chronic illness is coming on, becomes anxious and insecure. This may be taken for granted. But it may also be taken for granted that it would be wise directly at this point to accept and face the situation and begin to talk it over. Here the professional team could well take the initiative.

The continued force of the basic needs of the individual would tend to operate *through anxiety and insecurity toward frustration.* This is the diffuse, unpleasant, uncomfortable feeling of semi-conscious realization that some of these basic needs *cannot* be satisfied. When this appalling recognition occurs, there may be great emotional disturbance in the sick person, and those who are involved in the care of that person may also become distressed at the apparent failure, at least psychologically, of their efforts. Certainly here again every effort would seem to be justified in discussing thoroughly the situation with the patient in order to prevent "focusing."

A characteristic feature of frustration seems to be the tendency to "focus" the unpleasant and hostile emotional feeling involved on some individual close to the frustrated person. This may be a spouse, a child, a parent, a close relative, an attending physician, a boss, or anyone else with whom the frustrated person comes in close contact.

The "focusing" of frustration may follow a pathway of acute symptoms leading through *resentment*, in which the diffuse emotional feeling of frustration becomes sharply pinpointed on a specific person, who is then considered responsible for the emotional distress which the frustrated person may feel. If this "focused" frustration and resentment is not relieved by some explosive outbreak, or by some substitute manner of obtaining

emotional relief, as in play, it may proceed toward specific *anger*, with sharp explosive outbreaks. So far the situation may remain controllable. However, if the anger is not explosively relieved, it may become blind, as in *rage*, and the emotional drive is toward the destruction of the personal object focused upon, which may lead either to homicide or suicide.

On the other hand, the "focusing" of frustration may proceed along a more chronic pathway. Someone close to the frustrated person may gradually be focused upon as an object of *jealousy*. This might be a particularly easy sort of development for the psycho-dynamic drives in a chronically ill person. Such a person, as a result of the frustration of the chronic illness, might readily become extremely jealous of the health and comparative happiness, of one of those who are in close contact, in day by day care. Such jealousy might extend easily through reverberating feedbacks to become *hatred*, and thus under continued drive to lead to premeditated *vengeance*, with the ultimate purpose again of the destruction of the person focused upon, with the consequent tragedy of either homicide or suicide. In chronic illness one can't get as ready explosive relief as in ordinary living, so that there is more of a chance of the chronic picture of "focusing frustration" than in ordinary life.

The dynamics of the individual drives are such, it seems, that once started along a particular pathway, the reverberations generate force forward. If this is the case it again puts emphasis on the importance of early training in the promotion of directing the emotional needs of an individual toward self-knowledge, self-understanding and realization, an active effort toward the balanced satisfactions of emotional drives and needs, which would result in healthy mental and emotional security and equilibrium. This is Maslow's program of self-actualization.

If there is repeated evidence of symptoms of anxiety, insecurity, or frustration, there should be an effort, particularly in chronic illness, to change the direction of the forward moving forces away from the diffuse feeling of discomfort, dissatisfaction, and "out of sorts" which characterizes the anxious and frustrated individual. Every attempt should be made in chronic illness to prevent the diffuse feeling of frustration from "focusing" upon a person in the patient's environment, so that neither the acute

symptoms of resentment and anger, nor the chronic symptoms of jealousy and hatred, would tend to develop. It is important to do everything possible in chronic illness to help the sick person toward an active attempt to reach balanced satisfactions of emotional drives and needs, as far as the circumstances of the particular chronic illness permit.

CHAPTER III

PROBLEMS OF TEAMWORK IN THE CARE OF THE CHRONICALLY ILL

FACTORS IN TEAMWORK IN CARING FOR THE CHRONICALLY ILL

CHAUCEY D. LEAKE

The effective handling of the increasing load of chronic illness calls for extraordinary efforts in the development of teamwork among experts in many diverse fields. Remarkable success has already been achieved by such teamwork among various experts in child health and welfare. The broad pediatric program organized in this country has won warm response from people everywhere, and has provided a base for continuance into adult and indeed in preparation for old age. This program in pediatrics has involved research, organization, teaching, and then practical application along pragmatic teamwork lines toward the goal of optimum health, physical and mental, for children.

It is proposed that the example set in pediatrics for effective teamwork be applied in the care of the chronically ill. Prime members of the team would be competent physicians in all specialties, and clinical psychologists. Technical medical service from responsible physicians in caring for the chronically ill may now well be supplemented at a personal individual level by the special skills of clinical psychologists in testing, evaluating, and guiding toward optimum adaptation to the situation, both from the standpoint of patients and from the position of attendants. By developing a mutually satisfactory organizational effort, these medical and psychological experts may extend the team to include nurses, laboratory technologists, physical and occupational therapists, medical case workers, sociologists, public health officials, and hospital and institutional administrators.

Major factors in effective teamwork between physicians and psychologists are, (1) goodwill; (2) mutual understanding and

respect; (3) clear delineation of responsibilities; (4) joint effort in research, teaching; and (5) programmed application of mutually agreed-upon procedures and policies. In organizing and developing the medical-psychological team for the care of the chronically ill, much group pressure must be directed toward the maintenance of mutual appreciation of and regard for each team member's potential contribution and actual sense of in-group security. It is the actual working together in research, training and service which can develop the team's ability morale and success.

People with chronic illnesses do have many complicating psychological problems relating to themselves, their relatives, friends and attendants. Those who care for the chronically ill also have confusing psychological problems involved in the very care they try to give. Both patients and attendants need to know and recognize these psychological problems. Clinical psychologists, in testing, appraising and revealing such difficulties may be indispensable in the rational planning of satisfactory adjustment to chronic illness. Full coordinated teamwork with physicians can aid in getting the facts for planning, in the planning itself, and in carrying out the program. Successful handling of the psychological problems associated with chronic illness may be the key to optimum adaptation to the disabilities and thus aid materially in meeting the medical, economic, and social aspects involved.

In such an effort health training centers are vitally interested. Perhaps much could be learned regarding the elements of teamwork from the example set in the military health services. From conferences such as this, on local as well as national levels, may come the expanding policies which can be mutually agreed upon for succesful teamwork between the medical and psychological professions in the care of the chronically ill.

Toward such a goal it would be expected that strong public support could be obtained. The public seems to sense the necessity for such teamwork in connection with the rapid increase in chronic illness with a growing population and with an ever increasing number of old people. Let the public become one of the major players on the team.

SOCIAL PSYCHOLOGY OF TEAMWORK

JAMES F. GARRETT

This conference is devoted to the topic of teamwork and as such has demonstrated that it is conformist—it is following the dictates of fashion. It is axiomatic today to talk of the approach to the "whole person" and its corollary, the "team" approach. However, we have repeated these phrases so often to ourselves that we have unconsciously accepted not only the validity of the approach but also the premise that bringing together a group of different disciplines results in teamwork. It has become a platitude almost without meaning.

Actually the goal of teamwork has been well expressed by Shakow when he said that "The essence of true team activity is the coordinated thinking devoted to a case, thinking contributed by persons with differing points of view growing out of different training, rather than the specific and detailed study of each case by each discipline's specialized techniques." Thus, teamwork actually means that the group gets together and thinks as one with the client's needs as a focus. But you do not develop teamwork by merely getting the group together and exchanging reports. Nor do you get teamwork by interpreting reports to one another. There can be developed a thorough understanding of the client's needs and of the techniques necessary to develop his insight into those needs and his cooperation in a constructive program to meet them and yet not have a "team" approach due to the inability of the group to coordinate and cooperate in a joint plan.

Moreover, from the viewpoint of the client, it is important that this "oneness" be pursued as an aid in the personality integration of the client. Any deviation may set up diverse goals for the client by different staff members. The client may thus be unable to integrate all these different concepts into an integrated concept of himself. This in turn interferes with the client's "acceptance" of his disability. Hence, there must be consistency and harmony among the demands made upon the client in order that the client may have clearly attainable and consistent goals which result in that state we call "motivation."

The crux of teamwork, then, lies in the social interaction in the "team" situation. Thus it is that Whitehouse, in his excellent

series of articles on Teamwork, states that the ideal team member "must be able to work with others and develop firm relationships with them. He must be emotionally stable, secure, mature and flexible in professional judgment—a team player not a star." This has been so difficult to achieve that Whitehouse has wisely chosen the term "ideal" in his description.

To have a real team, then, there must be acceptance of other persons on the team as equals with contributions of the same importance as one's own with an equal voice in the discussion and the conclusions. There is no room for the intellectual tug-of-war, the jockeying for professional dominance or the preservation of vested interest. Moreover, the tower of Babel of peculiar professional jargon must give way to a universal language of thorough understanding of client needs.

All of this is difficult to achieve. One reason is to be found in the different social status and prestige of the various professions involved in patient care, as well as the problems involved in certain legal responsibilities. Thus, in a typical "team" setting of an internist, a psychiatrist, a psychologist, a social worker, a vocational specialist, a nurse, an occupational therapist, a physical therapist, and a librarian in a hospital setting, the factors of professional and social status and prestige (perhaps unwittingly suggested in my listing) become apparent. Usually the physician by tradition has the highest status and prestige, and, depending upon your profession, I suppose, the others group themselves in a downward scale. The internist, by dint of his training and experience—specialization if you will—feels more "at home" with the psychiatrist than with the rest of the team, and more "easy" with the social worker, the nurse, the occupational therapist and the physical therapist than with, say, the vocational specialist. He may even view the latter with some suspicion since he is usually not too clear just what it is the vocational person does or what his competencies are. Usually his knowledge is in the form of stereotypes. And the same observations may be made about other team members in their reactions to one another. Thus, "acceptance" of one another is a difficult process.

Moreover, the willingness to face up to the limitations of one's knowledge and that of one's profession is not readily found. Nowhere does this become clearer than in the team situation when

some of those of "lower status" raise penetrating questions in an effort to see more clearly the implications of their own knowledge and insight into client needs. To those who have operated in certain authoritarian settings, this apparent "challenge" sets up a reaction which destroys any efforts at teamwork.

Reading most of the articles about the team approach will also convince one that one of the vital decisions is who is the captain, the presider or the convenor. Usually the author finds no difficulty in concluding that the logical person to do this is a member of his profession. Here again the obvious factors of prestige and status, operating to defeat the very concept of teamwork, become clear, but the answer as to who does what is no easy one since the settings vary so greatly.

Instances of road-blocks to teamwork can be multiplied many times depending upon one's direct experience with the process in a variety of settings. There obviously is no royal road to success. It is apparent that the getting together of a group of experts to evaluate a client's needs and to plan a constructive program is no guarantee *per se* that teamwork will or can result. Acceptance of fellow team members is conditioned upon attitudes built up over a period of time and hence is a function of training and experience. How we train our physicians, our social workers, our psychologists to think about their own work and that of allied groups is crucial in the development of teamwork. But as important is the opportunity to engage in a cooperative and coordinative activity—words not reduced to action are meaningless. This then would appear to be the goal—training for appreciation of the other's role and experience in a joint enterprise.

Even so, we might raise question as to whether there are some who despite exposure to training cannot function as a team member—those who may be expert diagnosticians or clinicians operating individually but who cannot integrate themselves into a teamwork situation—stars but not team players. It is all too true that the personality of some does not lend itself to teamwork and the question arises—what to do?

So research might be directed to an analysis of successful "teams"—and those not so successful—to determine what are the factors making for effective teamwork. And then, once we have learned how to work among ourselves, we might address

ourselves to the question of the family of the chronically ill individual and the attitudes of the community.

TEAMWORK INVOLVES INTERPERSONAL RELATIONS

PAUL E. HUSTON

Since a focus of this conference is on teamwork in relation to the chronically ill, some comments about teamwork may be appropriate. In making such comments one feels that they represent a repetition of the obvious. Yet so many teams which start off in an auspicious manner soon fall apart. Some consideration of the team itself, in this conference, should be worthwhile. I have been particularly interested in research teams. What we say about them may apply to teams oriented toward service functions.

The purpose of a team is to produce a more effective result than can be accomplished by persons working alone. In the field of psychiatry team concepts have long been emphasized because of the complexity of the field of mental disorder and the variety of services required. Teams made up of persons coming from different disciplines suggest that these individuals who bring different skills, abilities, backgrounds, and attitudes to the team have their activities coordinated for a common purpose.

Research teamwork may be broken down into three phases, the planning, the execution, and the reporting of research. Here one can have various combinations. Research projects may be planned in concert but carried out separately and published by separate individuals as part of an overall program. Projects may be more or less planned separately, executed separately, and fused into a joint report at the final stage. Or, team projects may sometimes be very loosely organized, with individuals keeping in touch with each other from time to time at various stages of planning, carrying out the work, and reporting.

It is at once obvious that whatever the degree of relationship at the planning, executing, and reporting stages, a problem of interpersonal relationships arises. These interpersonal relationships are important regardless of whether a joint project is planned chiefly in one area or in an interdisciplinary area. If, for example, a project were planned by biochemists, other individuals may be brought into the project principally as technicians or consultants at various stages of the total operation. Research

in one area does not present the same difficulties and problems that arise when the project involves persons from several disciplines with their differing professional skills and attitudes. We have, therefore, to think about what is necessary for collaboration. Good collaboration involves a classification of common goals of the team; otherwise the purposes of the organization are lost. Good collaboration recognizes the basic professional differences of the several disciplines regardless of their particular activities; this promotes an individual's professional self-esteem. Good collaboration is participative, that is, every member of the team must somehow feel that he is participating in some phases of the operation in a significant way.

As I said earlier, difficulties arise in interpersonal relationships in team activity. We may list some of these. Our recognition helps avoid them. (1) In interdisciplinary projects an individual from one discipline may feel that the fundamental nature of the problem under study is such that his role should be greater. If he feels this way, he may be critical toward the project and create dissension. This particular difficulty is apt to become more acute if the nature of the project or activity changes during its life and an individual who originally had a high status finds himself of lesser importance for the enterprise. (2) Another problem arises when a member of the team is brought into the organization or the project where the activity is already structuralized and where he grows to feel that he cannot fit into the structure because it des not make an adequate place for him. He finds himself in a highly complicated situation, where his own professional needs are not being met and he is faced with the decision of withdrawing from the project or of continuing to work while his standards and ideals are compromised. (3) In research teams particularly, but also to some extent in others, there sometimes develops a distinction between the idea man and the practical man. The idea man tends to disparage the activity of the practical man, implying that his work is without imagination or is technical; whereas the practical man speaks of the idea man as impractical, a dreamer, or a person of vague ideas. (4) Research teams falter when one member begins to feel that his own work is more important than that of others in the group. He tends to look with disdain upon their activities and they, in turn, come together and present a united front against him and

accuse him of wanting to run off with the project. (5) Research teams disintegrate when one member develops a new technique, a new theory, or a new practice, which perhaps began as related to the central topic under study but which, as time goes on, becomes more remote from the central topic. He develops a strong desire to follow up this lead, believing that he can perhaps make a significant contribution to science or a name for himself. Thus his work becomes more isolated with the attendant feelings on both sides of his not belonging to the group or the project. (6) Teamwork fails badly if there is a "prima donna" personality or other types of maladjusted personalities in the organization. The prima donna has a strong need to dominate in order to keep his own ego intact. Teams dominated by a prima donna actually may produce highly significant work. Here there grows up a "pecking order" with a hierarchy of dominant and submissive roles being played by various members of the team. Such organizations, however, are not usually democratic in the best sense of the word and may not allow for professional and individual growth and maturity. (7) Teams are wrecked on status problems, such as professional ones. A person of lower professional status may become hostile because he believes that he does not have adequate recognition. In this setting cliques develop and struggles for power occur. (8) Teams are wrecked by the administrative sins of bypassing and undercutting. Here the failure to consult results in stepping on sensitive professional toes with attendant hostilities and conflicts.

In thinking, therefore, of collaborative work of any kind, one has to keep in mind devices which facilitate teamwork. There must be kept before the group a clear statement of objectives of the enterprise. There must also be a clear statement of division of responsibilities and the relationship of each person and his activities in the total organization. There must develop an attitude of respect for the other members as persons as well as their professions and their contributions to the total enterprise.

All this suggests that the phases of the work, such as planning, executing, and reporting be discussed jointly. It is valuable in this connection to have conferences at regular intervals to discuss the internal operation of the project or the service. In the case of research, progress reports from various members are effective, as is also the device of reporting papers to the entire group for

suggestions and criticisms prior to publication. Journal clubs oriented around the problems of the project and case conferences do much toward clarification of objectives and roles. A factor of outstanding importance is the personality of the director of the project, who has the job of keeping the work moving toward the main goal. He must himself see the relationship of one person to another and to the total project. He must be able to handle the minor interpersonal difficulties that develop.

Current discussion of teams sometimes tends to leave one with a feeling of confusion of purpose. A team, as I said at the outset is only valuable if it accomplishes the work more effectively than persons working alone. One sometimes gets the impression that the team is elevated as an ideal simply because of the team concept itself. Sometimes those who talk most about teamwork are those who feel their status in the team is an inferior one. It may be helpful to discuss teamwork more realistically in relation to some specific propositions such as, "Teamwork is better than individual work if the course of an illness is shortened or its severity reduced." 'Since teamwork is more expensive than individual work a team must justify itself on a cost basis." "The team leaders should be persons who by training are most competent to assume overall responsibility for the activities for which the team exists."

THE GENERAL PRACTITIONER: A ONE-MAN TEAM

William Rottersman

I am impressed with one theme touched upon by the preceding speakers, and that is the iatrogenic factor in the production and maintenance of chronic illness. The physician, in addition to reflecting the cultural bias of society, which includes the family, possesses certain narcissistic needs and needs for magical achievements. The physician has difficulty in accepting the limitations of his therapeutic efforts against illness, defect and deformity, and this attitude on his part contributes to the persistence of illness. The ophthalmologist who finds he cannot bring himself to tell his patients that they are and will remain blind is too often forestalling a feeling of despair and impotence in himself, when

he hesitates "to clarify his patient's visual status."[1] By doing so, he fails to help the patient accept his illness and learn to live with it. Thousands of physicians maintain a state of chronicity in innumerable patients for reasons similar to those just mentioned.

It appears to me that we are in danger at this conference of over-emphasizing the dependency status of the internist. I would like to avoid convincing the internist that he is chronically lame as regards reasonable proficiency in the field of psychological medicine, and that he will always require a considerable degree of outside assistance in handling the emotional problems of the great number of patients that he sees daily. I believe that if properly presented the appropriate material will find the younger members of the medical profession having considerable more receptivity and strength than has been appreciated heretofore.

The prime responsibility for the care of the millions of chronically ill rests and will continue to rest on the shoulders of the practicing physician. The expressed need of the internist for assistance in handling the psychiatric problems of the chronically ill and their families largely stems from the past and, to a somewhat lesser extent, the continuing defects in medical education, both in medical school and on a post-graduate level. Very few physicians have had the opportunity to be students of dynamic psychiatry, social anthropology, or sociology, despite the fact that every patient they see, whether or not they are aware of it, represents a tripartite diagnostic problem—physical, psychological, and sociological.

Not only does our present system of medical education neglect to train the physician to be able to handle the emotional aspects of chronic illness but, as was recognized by the 1951 Conference on Psychiatric Education, "chronic illness, disabilitiy and prolonged convalescence are seen only briefly if at all."[2] The feeling of incompetence that frequently besets the average physician when faced with the psychological problems of his patients will not begin to be solved by providing each physician or group of

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[1] Ft. "Some Psychiatric Problems in Rehabilitation Work with the Blind," by Louis Cholden, M.D. *Proceedings of the 27th Convention of the Am. Assoc. of Workers for the Blind.*

[2] *Psychiatry and Medical Education:* 1951 Conference of Psychiatric Education, page 38.

physicians with a retinue or team of advisors to assist him in coping with the sociological and psychological facets of his patient's illness. Such a solution is not practicable, because:

1. Almost every one of the 180,000 practicing physicians devotes a sizeable share of his practice to the care of the chronically ill. Certainly to occupy ourselves at length with the aid that can be furnished to this large number of physicians for assistance to individual patients on an individual basis by the clinical psychologist or, as the case may be, the social worker or adjunctive therapist, is to concern ourselves with one of the least fruitful ways of meeting the psychological and sociological needs of these millions of patients.

2. Doctor Harrower stated "there is no uniformly trained clinical psychologist in the sense that there are doctors, lawyers, dentists or educators."[3]

Many physicians are not able to define or understand the difference between a psychiatrist and psychologist, and are even less able to evaluate the training, experience and work of the clinical psychologist. At the present time, it is undesirable that physicians be assisted by clinical psychologists without the collaboration of the psychiatrist.

I assume "that the diagnosis and treatment of mental illnesses shall remain a medical responsibility," in agreement with the position taken by the American Psychiatric Association.[4] Concerning the practice of psychotherapy, I agree in principle with the recent remarks of a colleague, a senior clinical psychologist, who stated: "All too often the question of the ability to do psychotherapy and the right to practice it are confused with the ability to take full responsibility for the medical welfare of the patient and the legal right to do so. It is only when the conditions for practicing psychotherapy are those of independent private practice that these two functions become inextricably confused." I

[3] The training of the Clinical Psychologist, Macy Foundation publication, N.Y., 1947. In all fairness to the author quoted and to the rapidly growing profession of clinical psychology it should be pointed out that much has occurred since 1947, the date of the publication quoted, to introduce in all major universities relevant, meaningful and more uniform training for clinical psychologists. See in this connection also *Towards a Regional Program of Psychological Research and Training in the South.* Southern Regional Education Board, 1954—Ed.

[4] A Statement of the American Psychiatric Association's Position on Amending State Medical Practice Acts and Related Matters, March 7, 1954.

must point out that the statement is taken out of context. I believe that the adoption of recommendations here or at other significant gatherings that will further this confusion as to who assumes responsibility for the welfare of the patient can result only in lasting harm to the patient, and to the practice of psychiatry and the practice of clinical psychology. To avoid this, I advocate that all psychotherapy, for the present and for the immediate future, continue under psychiatric administrative control.

The rationalization of the average physician that his continued unfamiliarity with the psychological and sociological aspects of medicine is a consequence of his lack of time or opportunity to remedy this deficiency is often belied by the facts. Physicians, on the average, spend an appreciable amount of time attending hospital staff conference, county medical society meetings, and they subscribe to a number of professional journals. Last December, attending in Topeka a post-graduate seminar in psychosomatic medicine for general practitioners were physicians from all parts of the country. Inquiries of several physicians attending the program from Cincinnati with regard to why they had come to Topeka when they had in their own city outstanding authorities in the field of psychological medicine, elicited the astounding fact that these physicians were not even familiar with the names of these unusually competent workers in the field of psychiatry and psychosomatic medicine.

To date unfortunately, psychiatrists have failed to assume responsibility for educating the internist through the media I have mentioned, and others which I have undoubtedly overlooked. Very few medical facilities either use—or use properly and fully—the services of a psychiatrist or, better yet, the psychiatric team. The present trend toward the "solo" practice of psychiatry—the therapist, the patient and the couch, I think is unfortunate. I believe that increasing numbers of practicing psychiatrists should function as a quarterback for a (psychiatric) team which works together.[5]

I believe that the greatest of all contributions that the clinical psychologist can make at this time are in the areas of (1) testing

[5] Menninger, Dr. Wm. C.: The Relationship of Clinical Psychology and Psychiatry. *Bull. Menninger Clinic*, 14:1, January, 1950.

—psychological tests in connection with differential diagnosis, personality inventory testing, and the evaluation of the results of treatment, and (2) even more important, research in the area of mental illness. Dr. Walter Kass, Senior Clinical Psychologist at the Menninger Foundation, wrote me last week to the effect that "we need research in the factors that make for chronicity of illness and disability . . . While seeking to find out and control those factors which first break a man's capacity to function adequately, we have largely overlooked the inactivation process which turns him into a chronic invalid." The psychologist, because of his background and training, can provide in the area of research his greatest contribution. Certainly there is little work being done today to help us understand and to treat, either by research in the area of physiology or psychology, the basis for jelled homeostatis mechanisms that are resistant to verbal or somatic therapeutic pressure. It is by investigations of teams of psychologists, physiologists, psychiatrists into the factors that contribute to chronicity that the psychologist has most to offer for the patient, internist and others.

If we are to think in terms of realistic help for the large number of internists with their millions of chronically ill patients in the next decade, we cannot rely on the creation of a new profession allied to medicine and yet not a part of it, to provide the solution. To make the necessary changes in the school curriculum and secure the necessary faculties, train the special teachers, and then to graduate classes of these quasi-physicians who are skilled in psychological medicine, will not for decades provide any solution to our immediate problem. That does not mean that this solution, as well as others, should not be given serious consideration.

I like to consider chronic illness a continuum, at one end of which one finds the very severely ill patient (either physically or emotionally or both) and at the other end of the continuum an individual who is suffering from a mild degree of illness despite its chronicity. Factors which contribute to the illness are, of course, physical, sociological and psychological. The illness, (homeostatic decompensation), may be likened to the dipping of a pan of a scale because of the large number of weights that have been placed on it. One does not have to remove all of the weights all of the time for the scale to go up again, nor does one have to

remove even the largest weight on many occasions. There are a large number of patients in which the attitude of the doctor (the iatrogenic factor) is very important. It is influential not only with the patient but with his family, his employers and with the public in general. Improvement on the part of the physician as regards attitude and understanding of the nature of emotional ills and their relevance to the maintenance and exaggeration of chronic illness if communicated to the patient through attitudes and words, as well as to the patient's family, employers, and the public in general, can do much to lessen the pressures that tend to maintain chronicity in a large number of patients. The physician by an awareness of the sociological factors that make for chronicity by means of his professional and social position can influence the community to undertake new ways of thinking and handling chronic illness.

Much of chronic illness may be considered sociological; for example if an individual reaches the age of 60 and his earning power goes down from $100 a week to $70 a week, we consider him to be one-third disabled. If his earnings go down from $100 a week to $30 a week, he is obviously two-thirds disabled and chronically ill, a social responsibility, and the target of all those unwholesome ideas that go with such attitudes. The internist, if properly educated, can do a great deal to influence society in the direction of more wholesome thinking about the problems of chronic illness, old age, and all the other problems we are directly and indirectly discussing here today.

IS THE TEAM CONCEPT FACT OR FICTION?

Morton A. Seidenfeld

Dr. Berry, in some of the pre-conference correspondence, discussed in a very succinct and lucid manner his reaction to the concept of the "chronically ill" which is one of the major elements of the conference title. As I read this I wished someone had been equally eager to scrutinize more closely the other principal phrase, namely, "medical and psychological teamwork." Since to the best of my knowledge this was not done, I would feel remiss if I did not raise at least a few questions although I

could not hope to be nearly as successful in by efforts as Dr. Berry has been in his.

It is interesting that within the past decade the concept of "medical and psychological teamwork" has come to be quite generally accepted without very many bothering to see that it is used in a semantically accurate manner.

On the basis of impressions from a great deal of observation throughout the length and breadth of this country, and a good bit in Europe as well, it appears that few phrases have come to be so universally honored—in the breach—as has this particular concept. Let us, for the moment, scrape a bit of the epithelium off the surface and look at some of the raw histology and pathology that underlie the facts regarding the medical team concept in toto.

We are told that the patient is a total human being and when he becomes ill he becomes ill as a total human being. We cannot, therefore, look upon him in terms of his pathological tissues, his symptoms or his abnormal behavior alone, but must view him rather as a total individual reacting to one or many noxious stimuli which result from a disturbance of the homeostatic balance within his body, and between his body and the external environs. Since this is the case we are also told that his diagnosis, treatment and ultimate readaptation to life will be dependent upon our viewing him with reference to how the ailing parts are interrelated with the healthy parts. Our goal is that the patient as a whole will recover.

Theoretically this is all very fine and is probably sound so far as modern know-how has demonstrated. But the rub comes from the fact that our professional training, from the start and with very few exceptions, has continued to deal with deviation from the norm in a highly individualized and specialized manner. Pathology, physiology, histology, biochemistry, abnormal psychology and psychopathology, psychiatry, surgery and all its minor divisions, internal medicine, and the whole gamut of specialty subjects in the medical and cognate fields continue to be taught year after year in a manner designed to make the student learn and therefore believe that man and his behavior are and always will be compartmentalized in one fashion or another. And even where leaders in professional fields have, on paper and in broad concept, made advances that are designed to

lead to a more effective correlation and collation of the various interrelated elements, the teachers who can work such material successfully into their old-time lecture (often unrevised for more years than one would imagine) are few and far between. Sure, there are exceptions, but they are noted primarily because they are so few in number.

Furthermore, the advances that are made on one front are often retarded on another. For a single example let us consider the young physician who has been trained in the most up-and-coming medical school of this country. He graduates and goes out on a rotation interneship, followed by a residency, that may bring him in intimate and reverent contact with great members of his chosen profession who still see medicine according to their own lights, which by no manner or means must include the holistic views expressed by their contemporary and more recently graduated associates. Furthermore, these same young men frequently appear before specialty boards in their chosen area of specialization and find that little premium is paid for their ability to treat the whole patient and much is granted them in kudos if they know every technique sacred to their chosen specialty.

This is no slur on medicine as a profession for it is equally true of every one of the cognate medical services. I am sure that nursing, medical and psychiatric social work, physical therapy and occupational therapy, psychology and especially clinical psychology, to name but a few, have within their professional folds leaders who make great pleas for the importance of the patient or the individual as a whole person, the need for co-operation and intimate coordination of all professional services that are to be rendered the patient. But what is overlooked is that these teachers, these philosophers are not the practitioners in the field. They are talking of a hope but their counterparts, their representatives in the field, are often so busy grinding the axe that will insure them professional status that they hardly have time to see the patient at all, let alone as a whole person.

This could be stressed further but I shall refrain from saying more except to re-emphasize to this conference that before we can hope to deal successfully with the team service to the chronically ill or, for that matter, to the chronically well, we must seek for more adequate ways of establishing in practice an actual

team relationship between the physician and all those who are privileged to share with him the opportunity to serve the sick and the well. The present practice of labelling a collection of disjointed, poorly coordinated and often antagonistic services collected together under a single roof a medical team is as dangerous as covering over an atom bomb with a bed of roses in the hope that none may be the wiser. Teams grow out of mutual respect, mutual conviction that without each and everyone on the team being there, the effort would fail or be far less successful, and out of a willingness to sacrifice personal privilege for the good of those who are to benefit from the team. Let us review teamwork with this point of view in mind.

PRESENT DAY PSYCHOLOGY: ITS CONTRIBUTION TO TEAMWORK

JAMES Q. HOLSOPPLE

In a conference of this kind, one *cannot* hope to discover new facts, to resolve old and deeply seated issues, or to settle violent semantic controversies. What we *can* do, however, is to crystallize in conscious verbal form certain propositions which are present but not too clear in our collective thinking and which, if formulated, might be accepted as articles of faith by those who would rather move forward from them than remain trapped by discussion about them.

I offer one such proposition to this group. Medicine and Psychology are now, for the *first* time in history, in a position to merge their hitherto separate medical and scientific interests and skills in behalf of the chronic patient, so that he may profit as the acute patient in the last half century has profited by the concerted efforts of certain medical specialties with other branches of science.

There is little doubt that the great medical advances of the last half century have been a direct result of the recognition and acceptance by those whose primary social mission is to cure disease—the physicians—and at the same time by those whose primary mission is to acquire new knwledge—the scientists—

of the fact that there is a common area in which both groups have a vital interest and which can be preempted by neither group to the exclusion of the other.

I do not mean to imply that no physicians have ever been great scientists, nor that the scientific group has ever been devoid of great humanitarians. But the fact remains that there has been a disjunction, and in my thinking, an unprofitable one. We have the symbol M.D. and the symbol Ph.D. which emphasize the difference. We have no symbol for the area of agreement.

My comments bear particularly on the problem of mental disorder because I know this field best. But they apply as well to other problems of chronic disease. The research by psychologists in the areas of tuberculosis, neoplasms, gastric disorders, multiple sclerosis, and many other diseases shows real promise.

Why, then, has it taken a century for psychology, psychiatry, and medicine to approach the kind of teamwork which other medical specialties and scientific disciplines have found to be so profitable? The answer, I think, lies in large part in the fact that psychology has only recently acquired the necessary tools, techniques, and concepts for making a significant contribution. This contribution need not be exclusively psychologistic but can be shared, with adequate curricula, by other disciplines. For example, in the field of mental disorders, and with specific reference to psychiatry and the evaluation of therapy, a central and basic problem, I would note seven items which illustrate this position. I attach no importance to the order but list them as they occur to me.

1. *Quantification* of clinical judgments. Application and adaptation of the basic principles of measurement to the data at hand, through the development of valid and reliable scales.

2. *Consolidation* of an almost infinite number of possible questions into lists of finite length; the use of factorial analyses to reduce the dimensions of measurement to manageable proportions.

3. Development of new types of useful data for analyses and measurement. This includes *projective techniques* which provide a large body of relevant data from which conclusions can be drawn independently of conventional history and interview techniques.

4. Dependence upon *rigorous experimental design*—as it is applicable to some problems of chronic disease.

5. Clarification of problems of *criteria*, as may relate to improvement of regression in illness.

6. Continuing attack on *problems of theory* as it bears directly on critical medical issues.

7. Acceptance of requirement of interdisciplinary approach.

In dealing with quantification of judgments, both psychologists and psychiatrists would do well to rid themselves of certain notions which retard progress. Among the more conspicuous of these I suggest the following:

(a) The notion that a quantity which can be gauged by a stupid, insensitive person is somehow more "objective," more "stable," or more "valuable" *as a quantity* than one which can be estimated only by a bright, well trained, sensitive person.

(b) The notion that, because certain properties of clear importance certainly *cannot* now be quantified, therefore those which *can* be quantified should be assigned a lower place in the hierarchy of values assigned to properties.

(c) The notion that the ability to make valid estimates has something like a perfect correlation with one's status as a "psychiatrist" or "psychologist."

No one doubts that at the extremes of measurement a simple judgment correctly reflects change. The opinion of the psychiatrist that this person is "markedly improved" or "much worse" can be readily taken at face value. But here the corner grocer can also make a valid judgment. Our real problem lies in the intermediate part of our scales, and as the psychologist sharpens them the psychiatrist must refrain from arguing that the validity of his judgment at the extremes validates also his own private estimates of intermediate scalar points.

Factorial analysis is a powerful tool in the hands of a skillful worker. But it is also a dangerous tool. The psychologist sometimes seems to be unaware that nothing can come out of the factorial machine which was not put into it in the first place. Or, having been forced, however reluctantly, to baptize his factors, he is put on a paternal defensive because his progeny does not quite live up to the name of a distinguished ancestor. The psychiatrist, on the other hand, unaware of the virtues and vices

of factorial method, may alternate between unjustified reliance and a disappointed depreciation of the method as too statistical and insufficiently clinical. Here, I think, we can look forward hopefully to two developments: instruction of the psychiatrist during his years of basic training in the methods of psychology, and better instruction of the psychologist who is learning the method concerning the areas of its applicability.

Projective techniques, I believe, are only at the threshold of their real usefulness. They have, however, suffered from some serious handicaps. During the early days of the last war psychiatrists were created in days or weeks. Young physicians were diverted from all other specialties into psychiatry, and they entered without theoretical background or practical skills. Projective techniques looked like a quick and easy solution to many of their problems of diagnosis and they were sold by people who did not know what they were selling to buyers incapable of making a discriminating purchase. Undeserved general disrepute was bound to follow and it will take many years to live down the experience and live up to the hopes of such efforts as, for example, the Michigan Assessment Experiment. However, we now know, and as scalar clinical judgments become more precise we will know still better, that conclusions from projective techniques will comprise an irreplaceable component in evaluations of therapy.

Psychologists, of course, cannot claim sole credit for the introduction of rigid experimental design into evaluation of therapy. But they deserve much of it and can make substantial contribution by continuing to insist upon it. As recently as three years ago when we were planning our design in the VA for the evaluation of prefrontal lobotomy our most serious resistance centered around the question of controls. They were "impossible to set up," they were "unethical," they were "unnecessary," furthermore "they just wouldn't work." How rapidly this climate has changed was reflected in a conference of distinguished psychiatrists only a few months ago in discussing the setting up of a project in evaluation of surgical and psychotherapeutic technique. One of the most difficult obstacles cited to a design for psychotherapy was the very real difficulty in setting up adequate controls.

In dealing with problems of criteria psychologists may make one of their most valuable contributions. Historically psychiatry has never come to grips with the problem. Shall we consider patients "better" if they leave the hospital; if the medical staff says "improved;" if they go to work; if their families say they are "well;" if they lose the conspicuous symptoms of their disorder; or how do we weigh any or all of these factors in global judgment. For psychologists, finding the answer to this problem should be a familiar task. Criterion research, and the fact of multiple criteria should be within the area of competence of any research psychologist attacking the problem of evaluation of psychotherapy.

With respect to problems of psychological theory, we still have a long way to go. Perhaps the best we can say at this point is that our psychological theoreticians no longer formulate their systems in the security of academic cloisters but do consider the facts of mental disorder. When our psychological clinicians show the same willingness to challenge some of the doctrinaire theories accepted in the clinic we may expect still more rapid progress.

Concerning interdisciplinary approaches, it can almost be said that the weakness of psychology as a profession may be its strength in the evaluation of therapy. Scientific boundaries between disciplines have always been vague. Modern psychology as we know it was, throughout the latter half of the nineteenth century, the effort of scientists of whom few expected in their youth to be psychologists. It is fair to say that psychology itself is interdisciplinary in origin and composition. Thus we have a heritage which insures that we lose no face in recognizing that evaluation of therapy may well involve the political scientist, the sociologist, the anthropologist, and many others beside the psychiatrist and psychologists.

Such combined effort as we envisage here cannot, of course, be successful without certain concessions from all concerned—the psychologists, psychiatrists, and physicians. But I believe they would be made if we agree on the central point that human welfare is a combined problem of the physician *and* the scientist, and it can best be promoted by joint research, and by interdisciplinary training at the appropriate levels.

DIFFERENTIATION AND INTEGRATION WITHIN THE MEDICO-PSYCHOLOGICAL TEAM

WILLIAM T. LHAMON

In the rehabilitation and treatment of chronically ill persons the development of teamwork efforts in rehabilitation centers stand out as the most lively attempts to cope with this ever-increasing problem. One surmises that there are a number of reasons for this modern growth of teams. In the first place, the range of information required to treat competently an ill person has become so great that it is expecting oftentimes too much of an individual that he be so learned and so encompassing. Secondly, it is probable that there is an increased efficiency, a saving of time and perhaps a saving of energy for the patient as well as of the physician. Facilities can be gathered under one roof and used by more individuals; information can be passed more quickly and more accurately when a team has gathered together than when individual specialists operate separately. There is also, no doubt, a sharing of responsibility and a lessening therefore of the stresses upon an individual physician or counselor. Among the many examples of teams which come to mind are family advisory services, comprehensive care programs, institutes, group practice clinics, rehabilitation centers and so forth.

In some ways the development of teams has greatly broadened and expanded the number of helpful people and devices which have been brought to bear on disease; an example of this expansion would be in the care of children where teamwork between physician, social worker, psychologist is most thoroughly developed. In the actual operation of a team, one tries constantly to maintain and sustain group spirit. This involves attempts and actions directed towards a co-equal spirit among the group members. The team has a life of its own that is dependent upon mutual respect for the individual member's contributions to the teamwork. Insistence upon the definition of responsibility for individual services and sub-parts of the team, the tendency to avoid warnings that team members shall not be exploited or "used" by any other team member are examples of these efforts to insure the existence of the team's spirit. It seems to me that besides the formation of the teams to accomplish the task of proving a patient's condition that there is perhaps an additional trend or

influence at work in the development of teams, namely, the urge towards *integration*. There is a trend towards unity and an attempt to suppress over-specialization.

This tendency towards teamwork and the trend towards integration bring to mind several items from the theory of small groups which have been demonstrated in the laboratory by Bales. In the small groups, which we might think of as teams in our sense, studied by this investigator two roles were differentiated. Someone assumes the role of a "task leader" and someone else assumes the role of "integrator." Apparently, the life and dynamics of the group require a differentiation out of the group structure of leaders. Therefore, it is important that this apparently inevitable social fact be recognized in the operation of our teams of therapists. We will, in other words, do better if we accept the proposition that someone must lead the groups. We cannot in reality expect a leaderless and everywhere co-equal group of specialists effectively to tackle any given tasks of therapy with chronically ill patients. Nor is it usual for a patient, whether chronically or acutely ill, to give over himself into the care of a group. More usually, the person who is sick seeks to give himself into the care of a competent individual. My experience with groups indicates that this happens in groups also. Someone in the group is selected by the patient to be his therapist. Where this is not possible, dissatisfaction occurs.

Perhaps it is part of the social system that when we are sick, as patients, we must hand over to another individual our care. Perhaps the social system is not yet structured in such a way that the sick members easily give themselves over for care into the hands of a co-equal group. Furthermore, the social structure is such that in medical terms, working in medical areas, responsibility is given to the physician and must be assumed by the physician. This means that co-equal groups of physicians and other specialists such as psychologists, social workers, occupational therapists or others must work, if they are to work within the framework of our social system, under the direction of the physician who must assume responsibility.

Even more urgent, to my mind, is the problem of research. Our information in the field of chronic illness needs great enlargement. Research is at least as badly needed as the applica-

tion of our current facts on the care of disease. The trend towards integration with its suppression of specialization may at times work similarly with differentiation and this will be bound to injure the opportunities for research. The researcher most likely to develop a new point of view and a new technique or a new treatment or anything new is almost by definition somewhat eccentric to the usual frame of reference. He is, in a way, not easily integrated into a group because of his difference.

It would be extremely important that teamwork and the climate of opinion in which teams are developed and which they tend to generate, not suppress the eccentricity and differentiation so necessary for the new insights we need in research. Somehow patterns of operation in which a research mind will feel at home and yet in which the advantages of intermingling various disciplines and specialities in an effective team must be found. My only concern is that, in promoting teams and promoting integration, we not make too difficult the formation of a patient-doctor interaction or patient-therapist interaction and further that we not make too difficult that eccentricity and differentiation which is the essence of novelty and of creative research.

MEDICAL AND PSYCHOLOGICAL TEAMWORK IN THE CARE OF CHRONICALLY ILL CHILDREN

W. C. Davison

The care of chronically ill children[1] differs from that of adults in at least three ways. First, except for mental retardation and leukemia, and now even for leukemia, it is expected that the chronically ill child will recover. Hence the attitude of the pediatricians and other general practitioners is more hopeful than that of internists, surgeons, psychiatrists, and psychologists. Second, children do not understand the reason for being ill, and sometimes are bewildered and are rebellious. Third, in addition to all of the problems of chronically ill adults, children need

[1] Behavior and psychiatric problems, burns and other conditions requiring repeated plastic surgery, cancer, cerebral palsy, chronic severe asthma, congenital and acquired heart disease, leukemia, mental retardation, nephrosis, poliomyelitis, rheumatic fever, tuberculosis, etc.

tender loving care (TLC), education, and provision for growth and development.

The goal of the pediatrician and family physician is to help children recover rapidly, happily and hopefully without invalidism and maladjustment, and teamwork obviously is essential. Equally obviously, any team, athletic or medical, needs a captain. The team leader should *not* be selected because of having an M.D., Ph.D. in Psychology, or M.S. in Nursing. The choice should be made on the basis of having an instinctive love and understanding of children, and an ability, like that of Osler, to make each patient feel that everyone on the team is primarily interested in his or her welfare and is doing everything possible to hasten his or her recovery. These qualities seem to be congenital rather than acquired characteristics. The captain then should have absolute authority to set the pattern for the remainder of the medical, nursing and psychologic staff and students, to remove those who do not have the proper attitude toward patients, to permit mothers to stay with patients if he or she thinks they will benefit the children, to restrict the others, and to try to educate them to better mother-child relationships.

In some hospitals, this captain may be a physician, in others a psychologist, but at Duke, the leader is a nurse, Miss Mildred M. Sherwood.[2] That the staff has enjoyed and profited from Miss Sherwood's authority and example has been demonstrated by the unanimous vote of the two hundred and fifty former interns and residents to call their alumni group the Sherwood Pediatric Society.

This happy and efficient attitude toward chronically ill patients, which Miss Sherwood and other captains maintain, usually is based on an inherent instinct to do the right thing at the right time, a quality which some fortunate people have so that they do not need theories and rules. Others can obtain help from the publication of J. C. Spence and Grover F. Powers on the necessity of providing better psychologic environment with tender loving care (TLC) and understanding for hospitalized chil-

[2] While Miss Sherwood has been on leave of absence with the Atomic Bomb Casualty Commission at Hiroshima and Nagasaki, Japan, Mrs. Mary Lee Steele, Miss Sherwood's 1st Lieutenant, who has all of her instinctive qualities, has helped the team to maintain the same pattern and attitude toward chronically ill children, as well as to the other patients.

dren so that a hopeful desire for recovery is developed, and invalidism and maladjustment are avoided; or the papers of Harry and Ruth Bakwin demonstrating that hospitalized infants with prolonged diarrhea, malnutrition and metabolic disturbances gain weight and health much more rapidly if fondled by affectionate nurses and attendants; or the reports of M. A. Seidenfeld, emphasizing that chronically ill children, psychologically as well as physically, thrive better at home. The greatest handicap to this last plan, however, is the lack of sufficient visiting nurses, psychologists, physical therapists, social workers, and teachers.

It would be more efficient and would reduce the problems of the general hospitals if special wards or institutions could be maintained for children with chronic illnesses, such as Warm Springs, the Respirator Center at Houston, and the N. C. Convalescent Hospital at Greensboro for poliomyelitis, La Rabida Jackson Park Sanitarium, Chicago for cardiac patients, and tuberculosis sanitoria. However, most states do not have adequate facilities to have these children grouped together, so it becomes a problem of the general hospitals, each of which may have only three or four of these patients. In most of these institutions, the psychologic and educational care of these patients has been done by volunteer workers and not by a team. The calibre of the program has varied with the enthusiasm of the staff.

The educational problem is readily handled by school teachers assigned at certain hours during the day. There usually are enough children in each of the hospitals to require teachers of different grades.

Provision for growth is best maintained by an active department of physical medicine such as that at Warm Springs and the Respirator Center at Houston. However, most hospitals do not have these excellent facilities, and there is great need for the education of physicians in the necessity for simple physical and occupational therapy in order that children may maintain an active normal growth, muscular strength, etc., and not become physically stunted and weak.

The American Heart Society and other organizations have published suggestions for the recreation and amusement of these chronically ill children, which in most hospitals are administered by volunteer workers. Television is of great help in the care of

chronically ill patients, especially for those who are mentally retarded.

The six greatest problems in the medical, nursing and psychologic, or total or team, care of chronically ill children, in addition to the shortage of nursing and teaching personnel, are: (1) the education of medical students and physicians of the need for this total care and team work; (2) the selection of the captain, as well as the physicians, nurses, teachers, psychologists, social workers, scout leaders and other who deal with children on the basis of the previously mentioned instinctive qualities rather than on their degrees; (3) insisting that the team follow the captain's orders based on his or her TLC attitude toward children, instead of riding madly off in all directions like Lord Ronald; (4) assigning one member of the team to the patient and his or her family in order to avoid contradictory comments and reports; (5) training the family and others in what can and should be done for chronically ill children, and (6) having a definite plan of continuing and progressive treatment in which the patient, the family, and the team participate and evaluate.

CHAPTER IV

PROBLEMS IN TEACHING AND TRAINING MEDICAL AND PSYCHOLOGICAL PERSONNEL FOR CARE OF THE CHRONICALLY ILL

TRAINING THE PSYCHOLOGIST FOR WORK WITH THE CHRONICALLY ILL

ROBERT I. WATSON

In preparing this memorandum I make certain assumptions which should be made explicit. First, there is the assumption that it will be the consensus at this meeting that more extended training of the psychologist for work with the chronically ill is desirable and should be furthered. Second, there is the assumption that a person so trained makes a socially useful contribution, accepted as such by fellow psychologists and by other professional people concerned with this problem. Third, there is the assumption that some greater clarity concerning the role of the psychologist in this work will emerge during this conference making it more possible than it is at present to map, or, more important, implement such training. It follows that my task now is to sketch the present extent of training, or lack of it, and to suggest ways it may be done in the future.

To my knowledge there is no university in the United States which offers specific training of the psychologist for work with the chronically ill. This is an advantage in that whatever plans may eventuate from our consideration of the problem will not have to struggle with already existing and perhaps differing programs. As a background for such training it is profitable to examine briefly the general training program for the clinical psychologist. This general plan of scientific and professional training is the existing framework into which speciality training must fit.

The graduate program of training of the clinical psychologists is of four or more years duration leading to the Ph.D. degree. The first two years include a considerable amount of classroom work.

There is some healthy diversity from school to school, so I will hope to be forgiven if I center my description on what I consider to be one of the better programs, by curious coincidence that at Northwestern University! The first year is devoted to general experimental psychology, research design, and statistics. This year is taken in common, clinical and experimental students alike. It is training to be a psychologist, not specialty training. In the second year the clinical student devotes his time in the classroom to personality theory, diagnostic appraisal, psychotherapy, basic principles of clinical psychology, an introduction to medicine (taught by an internist) and clinical research design. Paralleling classroom work is clerkship experience in which the student begins to learn clinical relationships working either in VA or university hospitals. In his third year the student leaves the university for a hospital or clinic where he immerses himself in clinical work. This either may be a neighboring hospital or one quite far distant. Stipends, especially those supplied by the Veterans Administration and the United States Public Health Service, make it possible for the intern to be self-supporting. In brief, it is a period in which the psychologist-to-be works with patients in a team relationship with supervisor psychologists, psychiatrists and social workers. An internship as differentiated from a clerkship, shifts the focus of attention from techniques to the patient and his problems. It might even be said that the major goal of an internship is to make the student sufficiently adept so that his techniques do not stand in the way of his seeing the patient! In the fourth year the student often returns to the university, primarily to work on his dissertation, an original piece of research; but, in addition, he rounds out his training through special seminars, specialized clinical experience, and the like. Although not the most commonly used pattern, it is sometimes possible to work full- or part-time in some clinical installation while completing the dissertation. If any more than a modicum of time is spent in this fashion, it lengthens the Ph.D. program to five years.

Post-doctoral training in all aspects of professional psychology is definitely on the increase. In addition to post-doctoral training institutes and workshops there is the more important development of provision for post-doctoral appointments at various hospitals and clinics. They are not considered as staff appointments but are generally limited to one or two years.

For purposes, then, of this and later analyses of the problem it is possible to refer to training of the clinical psychologist as falling in the categories of classroom work; clerkships; internships; pre-doctoral, post-internship experiences, and post-doctoral training. Each of these categories will be examined briefly for their relevance and fruitfulness for training for work with the chronically ill. It is fortunate in this connection that graduate training in psychology is sufficiently flexible to permit changes to be instituted without necessarily having to move mountains in order to do so!

The first of these categories, classroom work in the narrow sense, does not strike me as a very fruitful or sensible place to introduce any particular emphasis on chronic diseases. These years are already overcrowded and, in general, are not suitable as a place for training in this area. Neither are the clerkships of the second year, since the student is still preoccupied with technique. The internship is traditionally a rotating one for the clinical psychologist in which he meets "all kinds and conditions of men." Thus, there is allowance for some acquaintance with this area, even in hospitals devoted to psychiatric disorders where most clinical psychologists today take their internship. The post-internship, pre-doctoral year or years, and post-doctoral training might be taken by a person desiring to specialize in work with the chronically ill in a setting such as a tuberculosis, cancer, gerontological or other hospital wherein chronic diseases are a major concern. This, in fact, has been the setting wherein students of mine have already worked, such as the St. Louis Infirmary Hospital or the M. D. Anderson Hospital in Houston. However, the training they received for this work has been fortuitous. A staff opening for research and for service attracted an individual who, heretofore, had perhaps given relatively little serious thought to work in this field. The administration of such a hospital was willing to take him in spite of little specific training feeling that he could learn on the job. The fact that this arrangement has worked reasonably well, is a compliment to the transferability of the training of the psychologist. It is, however, not necessarily the most adequate answer. At the moment, at least, I would consider that it lies in the setting up of both post-internship, pre-doctoral, and post-doctoral training programs in teaching hospitals.

CHRONIC ILLNESS FROM THE POINT OF VIEW OF THE TEACHING INTERNIST

LAWRENCE A. KOHN

"Chronic" means different things to different people. As George Berry says chronic disease isn't merely an illness which does not respond to molds or the knife. Nor is it merely an illness which is not rapidly progressive. Diabetes is a chronic disease, but except for those unfortunate patients who are under the spell of an obsessive physician, people live with diabetes, not by it.

Consider some of my everyday problems as a practitioner of internal medicine. One of these concerns the possible existence of specific emotional patterns as the result of specific disease states. Some years ago I was struck by the extremely nervous behavior of patients with cancer of the body of the pancreas, behavior much more disturbed than in many other equally serious disease states. This has been noted before but had not been explained. I was not able to explain it either. Probably a number of factors play a part, including the psychology of the physician, who, failing to make a diagnosis in spite of his best efforts, is likely to withhold the support which we all know is the keystone of treatment in oncology.

Dr. Harrower has studied multiple sclerosis by projective technics. Dr. Seidenfeld has explored the emotional reactions to polio. I am told that depression is fairly common very early in leucemia or lymphoma (Dr. William Greene). Has anything of psychological significance been found out about arthritis, or endocrine disorders, or heart disease, or ulcers, or bronchiectasis? A great deal of psychosomatic medicine is concerned with emotional characteristics which might influence disease. We somatists, and there are many of us here, are more interested in what lasting illness of varying types does to the human involved.

Everybody here is apparently concerned with the problem of adaptability. Since some of us are already in the aging group, we certainly should be. The problem however is far from uniform. The maintenance of useful living is a very different problem in serene old age, if there be such a thing, from what it is in the sudden crippling medical or surgical accident which may happen at any age. Some of this relates to my first query; it

seems that paralytic polio and multiple sclerosis bring out quite dissimilar personalities. There has been some study of amputees, but what of people with colostomies? Perhaps here also we doctors help determine the behavior of the patient.

There is marked adaptability in the veteran. I have been struck, in consulting and teaching at a V.A. installation, by the compartmentalized attitude of the physicians who work there. A problem is treated as either medicine or psychiatry. Some of this is due to inadequate education, of which more later, but some of it is because of scanty psychological information about patients who are in the unusual situation of being chronic by a combination of free choice and the American Legion. Hemingway tried to describe a bit of this in one of the earlier novels,—*To Have and Have Not.* And when the bill is written for the veterans, what group of doctors is most able to fill it? Should those of us who are working in the open attempt from time to time to make the transition to the twilight of a Facility, or should the choice of physicians be from those who in war and post-war have developed scars similar to those of the inmates?

Besides practicing medicine, I also do some teaching. I can assure any of you who are not actively engaged with students and house staff that it is a constant struggle to keep the young from inducing or perpetuating disease. One tries to establish the attitude that dealing with a patient resembles the problem of preaching at a funeral; what you try to do is to make everybody leave feeling a little better about a tragedy. But youngsters equipped with tests somehow manage to find something the matter with all of us. When there is no border-line hydronephrosis nor brucellargin skin test, there is always the psychological side. As for the young surgical specialist, both his emotional and financial needs seem to require the creation of anxiety. I am aware that the patient has to have a need for anxiety in order that it be established, but I hope that the psychiatrists in the group will agree that anxiety-proneness resembles sources of focal infection; just keep looking hard enough and it will be there. Of course the easiest thing in medicine is to note the anxiety-producing proclivities of the other fellow, so let me hastily say that I do not always know when I am exceeding the limits of support for patients and begin to make them dependent. Returning once more to my first question, more

information about the susceptibility of various disease entities to over-dependence would help to draw the lines and to teach their recognition.

There is more to teaching than that, of course. The medical profession is a cross-section of the community in intelligence as well as integrity, regardless of how carefully we select students. The psychiatrists among you will doubtless add that we physicians also exhibit all the general public's range of emotional needs. I believe that the tools which we give students to work with must be simple and within their capacity to use. That is after all the American medical tradition; bring the complex test or therapeutic technic to a stage where everybody can use it. At the risk of being contradicted, I think that on the psychiatric side the tools are still too complicated for many of us. To teach dynamics is enormously important, but teaching the average student when not to apply his half-learned lesson is equally important. Recently a very young psychiatrist brought out with skill that an elderly man, in a state of confusion and excitement, had just moved to Rochester and was lost and disturbed without his familiar medical advisor. It was true and it was important; just what was to be done about it was not made clear. Unfortunately this formulation ignored the existence of fever, probably due to a patch of pneumonia, and a possible small cerebral thrombosis. Of course the medical man could not cure that in a hurry, either, but at least he put the patient to bed and treated the infection rather than sending him back to Buffalo. I harp and nauseam on this simple theme; don't try to use a tool with which you are not familiar, whether it be in surgery, in physical diagnosis or in handling emotional illness. The maximal effort of our teaching should be in the areas of maximal productivity of simple tools.

My last question concerns transference. I hope the psychiatrists will accept the use of this term as a general statement of what should happen between any doctor and his patient, and not in the strict analytical sense. Just how does the patient at various ages accomplish *multiple* transferences? This is difficult for the patient who has been under medical care even in such a relatively simple situation as the sudden need for a good relation with an unfamiliar surgeon. When a patient sees a psychiatrist as well as a medical man, transference problems become highly

complex. This has importance in any plan for teamwork. In the aged or the chronic the choice of doctors is much less free than in earlier years or in acute illness. There are practical reasons on both sides which tend to perpetuate the relation even when it is not satisfactory, even when the magic is no longer potent. Perhaps conscious considerations are relatively more important in chronic disease, such things as interest and faithfulness, even of cost may count more heavily. This question is open to experimental approach. Finally, in chronic disease the patient must constantly relate to other figures than the doctor; to the family with whom he lives, or to the house-mother in the convalescent home. Should the doctor's role then be played largely behind the scenes, be merely one of guidance, as an experienced clinician handles a ward, leaving the house-officer as the key figure? Or with so much routine, with so much waiting for the axe to fall, with so much despair, may not the doctor have to be the chief source of encouragement and hope? The sacerdotal function may be forced on him whether he seeks it or not.

THE MODERN MEDICAL CURRICULUM: EDUCATION FOR TOTAL CARE

George C. Ham

At the outset, I would like to quote from the initial letters in regard to this conference in which it is stated "The purpose of the conference is to bring together for informal discussion and exchange of ideas a representative group of physicians, psychologists and psychiatrists. . . ." And again: "This conference has been suggested by the expressed need of internists in obtaining aid from psychiatrists and psychologists in the care of the chronically ill. Current recognition of the many urgent psychological problems facing the chronically ill and their families *for which internists are not fully prepared* and *for which psychiatric personnel is not available*, indicates teamwork between internists, psychiatrists and psychologists as the best way of meeting a rapidly developing serious situation. It is hoped that an informal interchange of ideas between representative leaders in these fields may bring proposals for effective cooperation among all concerned in handling chronic disability in a more satisfactory man-

ner than is usually the case." In another letter eleven general subjects were listed as "likely to be discussed." Item Five states "Factors in the development of teamwork between *internists and psychologists* in the care of the chronically ill." The subject headings that follow in the letter have to do with the implementation of only this limited teamwork as described under Item Five.

Listening to the discussion of others during the first morning and in succeeding meetings, it seemed that the stated purpose of the meeting, namely, "This conference has been suggested by the expressed need of internists in obtaining aid from psychiatrists and psychologists in the care of chronically ill," had been narrowed somewhat to a discussion of the role of clinical psychology in the "care of the chronically ill" in some unspecified relationship to internists. Likewise, it seemed that certain assumptions, although unverbalized, were being made, namely, that if the clinical psychologists were able to work closely with internists that many of the problems of the chronically ill would be solved. It was assumed that the internist knew little, if anything, about treatment of the person, and that his training and interests were largely directed to the bodily or somatic aspects of the disease, and that the addition of the clinical psychologists would allow treatment of psychological and cultural factors which are being largely overlooked. Another assumption was in evidence, although unverbalized, that clinical psychologists and psychiatrists were in a sense equated if one excluded the obvious organic and toxic diseases which were still the responsibility of the physician-psychiatrist.

There was also an assumption present which delegates the other members of a "team" who are now involved and will deal with chronically ill patients, such as the social workers, sociologists, nurses, recreational workers, occupational workers, rehabilitation workers, physical therapists, secretaries, ministers, etc. to a relatively minor role.

It is my feeling and experience that in beginning to discuss such problems that a rather clear definition of the responsibilities of the various disciplines that are dealing with ill health is essential. These responsibilities should be based upon the training of the particular individual and the scope of his discipline. From this there can be derived a definite role for each of the members of a

group who may be dealing with individuals or groups of patients, which will be based upon their particular competence.

In attempting to define something as general as this, it is necessary to understand clearly that there is a wide variation along a spectrum not only in the training but in the ability of individuals within a given discipline, and this is dependent upon individual potential as well as upon the type of training that they have had. This applies to the rapidly growing field of psychiatry, in which there are people who although well trained in medicine, have not had adequate training in the psycho-therapeutic aspects of neuroses, psychosomatic disease, behavior problems, and psychoses. At the other end of the spectrum, rapidly being implemented, I am happy to say, is the situation in which a physician with broad training in the medical sciences, an internship, and three years of training in basic psychiatry in clinics and out-patient departments is receiving additional training which reduces his own emotional and intellectual roadblocks through personal psychoanalytic experience. This process opens his mind to the acceptance and integration of broad areas of human knowledge which in turn can be synthesized and applied to not only the treatment of illness but to the understanding of the meaning of illness to Man.

This same spectrum applies in the field of clinical psychology, but, in the opinion of this writer, the training programs in clinical psychology do not prepare their candidates adequately for the responsibility of reconstructive psychotherapy. Only in unusual individuals are they conscious of their psychological blind spots. This makes objective evaluation of the fundamental or primary or deeply unconscious factors in their patients relatively impossible. Therefore it is my feling that some of the assumptions which are inherent in the conference need critical examination so that the role and responsibility of the individual members of the working team can be sharply defined. In this regard, there is no question but that the well trained individual of good intellectual potential in the field of clinical psychology has a great deal to offer.

Of great value is the ability to apply "quantitative" and "semi-quantitative" and "objective" test procedures which are reproducible. (It is important, however, to point out that the interpreta-

tion of the "reproducible" tests varies quite widely in the field and in most instances has borrowed largely from modern dynamic psychoanalytic theory for its frame of reference.) Likewise most candidates for the doctorate degree have had some critical experience in regard to research design and evaluation, and many are therefore useful in this regard.

At this point, if I may, it would be interesting to interpolate an aside which has arisen out of discussion in many areas in regard to the problem of "research," and to point out that actually there is a great deal of scientific snobbery evident in many academic spheres. There appears to be a pecking order as to who does "real research" and the judgment in this regard depends entirely upon from what platform the individual seems to be speaking. It has been stated by men working in the humanities that they are the ones who do "real research," and their feeling is that social scientists and medical scientists do not really do "research." This same attitude has been expressed by social scientists who from their pinnacle look down upon the humanities as well as the medical sciences.

It must be pointed out that many physicians, through the implementation of scientific method, have made rather remarkable progress in the past 100 years. These groups of physicians pride themselves on having certain organizations, such as the Society for Clinical Investigation and the American Psychosomatic Society, which have strict and rigorous criteria for research. The only ones who seem able to stand undisturbed upon a solid foundation without any trembling are the physical scientists who have produced for us the remarkable phenomena of the reproduction of the power of the sun on earth.

One of the paragraphs from an early letter in regard to the conference states, "many urgent psychological problems exist facing the chronically ill and their families for which *internists are not fully prepared* and for which *psychiatric personnel is not available; (this)* indicates teamwork between internists, psychiatrists and psychologists as the best way of meeting our rapidly developing serious situation." It is true that many medical curricula up until the last few years have not adequately taught the physician during his medical school years the attitude of implementing and understanding the psychological, psychodynamic

and social factors involved in human development and adaptation. Deans of medical schools and others interested in the medical curriculum have faced this problem, and at the present time vigorous activities are proceeding in this country to see to it that medical students from the first day of their entrance into medical school are shown the broad aspects involved in "illness." Their horizons are constantly broadened in regard to the factors involved in illness under the headings of sociological, psychological and biological factors.

In some medical schools contact with patients is begun in the very first year in group and individual form. In the second year, departments of psychiatry in collaboration with social scientists and clinical psychologists are teaching courses in human development. These courses cover phylogenesis, ontogenesis, and the problems involved in the integration of the personality and character as determined by post-natal biological development, experiential factors, and cultural impingement, and opportunity. This training is continued in practical applications in actual diagnosis and treatment during the third and fourth years.

In some medical schools as much as seven full weeks is spent on inpatient psychiatry and seven full weeks on outpatient psychiatry under the 'teams" of psychiatrist, clinical psychologist, social worker, and social scientist. In addition, there is constant emphasis on the psychological and cultural factors while the medical student is on other services than psychiatry, such as pediatrics and medicine, surgery, obstetrics, gynecology, etc. This same attitude toward training is continued through the internship years and on into the residency years.

Thus large bodies of physicians are receiving broad and fundamental training in the factors of human reaction as a total person, and are being taught techniques of therapy under the headings of *"What Can Be Done, What Should They Do, What Should They Not Do."* In addition, training programs for specialists in psychiatry are implementing all of the factors mentioned above at a high level with the assistance and collaboration of behavioral sciences but with particular emphasis on personal psychoanalytic experience for the specialist physician, and continuous supervision of actual experience in dealing with all types of problems and types of human adaptation.

In most medical schools steps have been taken and are being implemented which will improve the ability of all physicians and which will increase the number of well-trained psychiatrists in the field. Although the above-named personnel will be educated to the understanding of the ability and limitations of clinical psychology measuring techniques, they will constantly need the assistance of this personnel in helping to evaluate patients' problems through test procedures, and to assist in following progress through these same techniques. Likewise, the need for assistance in the research design, and evaluation of data will continue. However, in my opinion, there is little question but what the future development and increased understanding of the problems of total adaptation of individuals and the application of therapy and prevention should and will remain in the field of medicine and in the hands of the physician.

Man is fundamentally a biological organism representing the most complex and adaptable of all the living species. In his high development he has acquired certain outstanding abilities, largely represented by the higher nerve centers, and certain handicaps. The development of the ability to remember, to associate experience and feeling and memory, to integrate, to synthesize has, in addition to giving Man his tremendous capacity for adaptation, also furnished the unfortunate tendency to misinterpret, misidentify, to integrate and synthesize in a neurotic manner and to solidify these warped values as fundamental building blocks of his personality and character which in turn effect the total biological operation. The understanding, in the research sense, and the treatment of this complex system, requires a detailed knowledge of biology in association with a fundamental and increasingly microscopic knowledge of not only the overt, but of the latent, repressed, pre-verbal, and possibly pre-natal "memory traces" and experiences of Man. "The danger inherent in separation of psychology and the psychological aspect of Man from the biological or somatic, as will happen if it becomes the general assumption that "psychologists deal with psychology, and physicians deal with biology" is to my mind a serious one, comparable to the gradual separation and compartmentalization that has occurred in medicine during the past fifty years into specialities in which narrower and narrower responsibilities and views have been

taken, and the total Man has been lost sight of. This latter problem has been recognized very strongly, and the medical curricula and the attitude of medicine are changing this situation rapidly. This in no way means that all disciplines having insight into the problems of Man cannot work individually and collaboratively, but that the understandings gleaned by any special discipline must be fitted into the spectrum of Man as he really exists as a biological, psychological and social organism with a very complex integration.

It is to be hoped that the application of our present knowledge as preventive techniques, starting with pre-natal clinics and the early treatment of the neuroses of young children and newly married people, and the early detection of the disturbances and distortions in their children will gradually reduce the incidence of "psychosomatic" illness as represented in chronic illness. This will come about through the training program of medical students and doctors and more and better psychiatrists, as well as the implementation of teamwork of all people involved in dealing with ill health, such as clinical psychologists, social workers, nurses, etc.

Because there is a lack of a certain type of personnel does not mean that one should replace it with someone who is not adequately trained to do the job. At this point I refer back to the assumption that clinical psychology is equated with treatment, and feel that it is essential to point out that any intelligent person can counsel someone on the conscious level; however if one is to understand the total meaning of the problems of the individual it must be thought of in terms of comprehension of the non-overt needs and personality techniques as understood in modern dynamic psychiatry and human development. This can probably be best described under the heading of the *Vicissitudes of Empathy* which involves the definition that it represents the ability to be objective, knowledgeful, humble and interested, and to be able to identify with a given patient without a disturbing reaction in the treating person. Except in unusual circumstances the attainment of this state is difficult and requires the investment of a rather large amount of time on the part of the treating person in discovering his own aberrations derived from his own life experience. At the present time this can be obtained only through personal

psychoanalytic experience. This technique by no means guarantees that every person who is exposed to it in a training framework will acquire this ability.

The high standards of the training institutions in rejecting and terminating the training of significant numbers of people indicates the nature of this problem. In essence, understanding and "emotional" acceptance of what are known as tranference attitudes on the part of the treating person are essential for maximum understanding. Knowledge gained through training and experience in the handling of transference problems is essential for other than advisory counseling, and is essential for reconstructive therapy and for deep understanding of the process involved.

In closing, I wish to make the point at the present time our insights are far greater than we have been able to transmit to others. Heavy emphasis should be placed upon the transmission of this knowledge to those people to whom patients are going to come, by tradition and who have been given the legal responsibility for treatment, namely, the physician. Such a program will go a long way toward achieving the goal of improved understanding and treatment of the chronically ill. In addition it will clarify the definition of responsibilities of the many disciplines involved in this and other medical problems.

QUESTIONS ON CHRONIC ILLNESS CONFRONTING THE MEDICAL EDUCATOR

CARLYLE JACOBSEN

One brings to a conference such as this the interests and problems one faces day by day at home. My concern is that of planning a program for medical education at two new medical centers. The discussion at this meeting has immediate significance in arranging brick and mortar and defining the staffing pattern. The handling of chronic illness and the kind of team needed, are pressing matters.

May I list some questions which I hope the group may wish to consider:

1. What is chronic illness?

2. To what extent are the problems of chronic illness not immediately medical? To what extent are they problems of social and personal adjustment, and of maintaining motivation of the patient, and of the physician (to care actively for the chronically ill)?

3. To what extent is the management of chronic illness a task of changing attitudes toward the chronically ill among physicians, family, employers?

4. In these efforts to change patient and community attitudes, what role do social institutions other than medicine play? The church, welfare agencies, the schools?

5. How can professional workers be prepared for serving the chronically ill? The medical student, psychologist, the minister, the teacher, the social worker?

6. Before specifying where and how to prepare, what are the unique contributions and responsibilities of these several groups?

7. How shall the necessary changes be made in that educational monolith, the curriculum? Should they be attempted radically or gradually?

8. Is the care of the chronically ill a unique aspect of medicine (essentially a sub-discipline) or should it be part of good medicine as taught and practiced generally by good physicians?

To these questions the consensus of developing opinion will be of utmost importance. This conference can do much in guiding the public generally in understanding the problems involved, and to take such steps as may more satisfactorily than is the case now, handle the growing load of chronic illness.

It seems important to define carefully the goals and aims of modern medicine. With the techniques and social outlook now available, there is a gradual swing in attitude from the proposition of waiting until sickness comes, to the matter of preventing illness, and to the promotion of optimum health. If the goal of modern medicine is optimum health, is then chronic illness any condition in any individual which is below optimum health for that individual? Aren't we all a little sick? To strive toward an optimum implies continual effort. With health as our greatest

asset, the promotion of optimum health would seem to be a worthy ideal.

Chronic illness involves many peculiar psychological problems confronting both the patient, the patient's friends and relatives, and all those who may attend the patient. It would seem that psychological programs might be developed to help attain as satisfactory a social personal adjustment of a chronically ill patient as possible. This implies continual motivation for as much rehabilitation as possible. It also implies continued positive effort on the part of the attending physician and the associated medical staff in understanding the situation and attempting to make the most of it. There is evidence of a changing attitude toward the chronically ill among physicians, the public generally, and employers. The extensive rehabilitation program proposed by the Baruch Committe on Physical Medicine, under the influence of Doctor Howard Rusk, has shown that a heavy financial burden on society can be alleviated by intelligent and well directed rehabilitation efforts. The program proposed by Doctor Rusk has been implemented in a number of cases with success. No longer is it felt that chronically ill patients should be put away where they are not in evidence, and where mere custodial care is enough.

In these developments in changing patient and community attitudes, many social institutions are involved. Physicians are gradually learning to utilize the great aid possible through the church, the schools, and welfare agencies. Similarly physicians are learning to utilize the special skills of psychologists, ministers, teachers, social workers, and physical and occupational therapists. Special nursing for the chronically ill may become one of the most important factors in successful rehabilitation. Gradually these various professional groups can learn the ways by which they may more satisfactorily be integrated into teamwork for the benefit of the chronically ill. The training program that will be concerned will develop on the basis of experience.

It would seem that the care of the chronically ill should be a part of good medicine as taught and practiced generally by good physicians. Here is a major responsibility for medical schools, in a generally neglected area.

REPORT OF STUDY GROUP ON TRAINING AND TEACHING*

Members: Kubie, *Chairman;* Watson, *Reporter;* Berry, Davison, Ham, Jacobsen, Holsopple, Brown and McDanald.

Holsopple: What is the chronological point at which psychological training becomes a problem? Is it at the level of the medical curriculum, the undergraduate curriculum, or the high school curriculum? Where in point of time ought we to start thinking about training?

Kubie: When you say "training," you mean training for what?

Holsopple: Training with the goal of getting an integrated attitude toward medical problems using psychological as well as psychiatric services.

Berry: The objective, it seems to me, is much like the objective in the whole of the medical school. Here we have a variety of trainings, all focused on the student, all the same in their relationship to him. The realm of medicine is no longer the property of physicians, but has become the wide area it is because of incorporating other disciplines, with all kinds of professional groups contributing. Many things make up medical education, but all focus on the common objective: the student. One understands by modern medicine something far beyond the contribution of just the physician. We must have an understanding of the patient's *psyche* as well as his *soma.* You must bring together a group, each of whom can contribute to this thinking. How do you give the psychologist an opportunity to continue to advance rapidly the field of understanding man and at the same time bring him into a relationship with the patient leading to a broader understanding of the patient necessary in order that medicine can help him more effectively.

Kubie: One can teach people enough about each other's discipline so they can understand one another. You can try to teach them so they all do some fragment of one another's job and become interchangeable parts of the machine in some measure. To what extent can one understand something in which one has never actually played an active role? Understanding had to tran-

* The Report of Group 11 is given in terms of their (virtually) verbatim discussion, which owing to the expert secretarial help they were able to "play back" to the group as a whole.

scend the obstacles that arise through feelings of jeopardy of one group by another. Progress, to be effective, has to take into consideration functions that are to be performed plus an understanding of one another and of the extent to which groups trying to establish rapport are threatened by each other.

Davison: In dealing with patients, one man or woman must take the lead. More harm than good is done if too many people ride off in every direction.

Brown: Will we improve the situation by the integration of psychologists into the teamwork in a more organized fashion? We must keep the patient-physician relationship foremost and work from this point in various directions but constantly circling back to the single physician and the single patient.

Kubie: We are facing a series of questions. Can we deal with both the organizational problem and the educational problem? Are we forced to deal with the two together, both in hospitals and outside, in order to keep the patient in the center of our deliberations? We have two extremes. At one extreme would be a limited concept of the role of the psychologist as an aid to the psychiatrist, who in turn functions with the internist, the internist as leader of the cooperative effort in the care of the patient. At the other extreme would be the psychologist trained essentially to the same degree as the psychiatrist with respect to all psychiatric problems, and functioning on an equal level with the psychiatrist. Between these two alternative poles are numerous intermediate positions. Our time concept of teaching and training must depend upon whether our goal is the one or the other or any of the many intermediate positions. It may clarify our thinking to hold this in mind.

Watson: There are arrangements in various hospitals where there is no referral step from the referral source through the psychiatrists to the psychologist. There are other departments than psychiatry where the psychologist might be attached. Can we not trust our colleagues to make appropriate referrals at the appropriate time or must we go through the psychiatrist in all instances? Even in a setting where the psychologist is attached to the Department of Neuropsychiatry, we must remember they build up other relationships besides referrals. Research often is

carried on by the psychologist with the neurosurgeon, pediatrician, etc., without interfering with this.

Ham: The problem we are considering today has been the problem of medical education since its beginning, with the addition of new disciplines. It is a fact that our psychological consultants are highly trained people and bring to bear techniques not available from anyone else. A well integrated staff knows when to ask for the help of the psychologist and calls him directly. In some cases people who did not have definitive training in psychotherapy but who had excellent training in basic experimental psychology and clinical psychology became the psychiatrists for quite an area. They did help people, but the real issue that came up was that they wanted to practice directly to the public and within the framework of the medical school, even though there is no licensure at the present time. They were not willing to accept referral of patients to them within the framework of the medical school. This led to an unfortunate situation where we were being pressed to allow them to do something without licensure in a state institution. However, these problems are being resolved and cooperation between all groups is being established.

Kubie: You are saying essentially that where the psychologist functions as an equal in the Department of Psychiatry, the situation has worked out satisfactorily. He has an appointment in the medical school, as well as a hospital appointment and an academic departmental appointment. Thus he acts in an appropriate academic manner.

Jacobsen: The day may come when there will be set up a Department of Behavior which may be comparable to five or six basic science departments. This would be somewhat more comprehensive than the discipline of psychology. That would give these people a definite status in the whole picture. You might find a home there for a number of people not as immediately involved with the issue of therapy. Those involved with therapy might find a better home in the Department of Psychiatry.

Holsopple: Here at Galveston, where they have set up a Division of Psychology in the Medical Branch, they have, on the contrary, made the Division of Psychology for teaching purposes a

segment of the Department of Psychiatry and for clinical purposes a separate hospital and clinic service.

McDanald: In Galveston the Department of Psychiatry is beginning to realize that although it perfunctorily uses psychologists as testers—it uses them as therapists, too, and observers—that it frequently does not understand the psychologist's communications of his test results and how they may be integrated into the overall evaluation and treatment program. Dr. Kubie's remark that we need to teach each other enough about our respective disciplines so that we can understand one another is especially appropriate to the Galveston situation. I would say that for psychologists to gain an enduring position on any therapeutic team, psychiatric, medical, surgical, or pediatric, they need to educate their potential collaborators in the various medical disciplines about what they can discover via their specialized techniques about people, sick or well. If the educational approach is tactfully utilized there may be some possibility of meaningful teamwork between psychologists and the medical profession in the not too distant future.

Berry: The minute you think about these problems narrowly, you put serious restrictions on training programs. The most important contribution a clinical psychologist can make is to advance knowledge in this basic field in which medicine is broadening rapidly.

Brown: The inherent danger is the increase in prestige and responsibility without proper training to visualize the whole.

Ham: The experimental psychologist looks down his nose at the clinical psychologist who is oriented toward patients and not research. This is pressing some psychologists to move over into what is considered a therapeutic area.

Holsopple: This is a squabble within psychology, but when outsiders come in, you'd be surprised at the solidarity of thinking which is presented!

Kubie: The pressure of the human need is so great that once the psychologist is clinically trained to any degree it becomes quite impossible for him to go on saying no to pleas for help.

Fremont-Smith: The internist has not appreciated either the need of looking at the human being as well as the organs he carries or the ramifications of this need into the family. The

internist neither recognizes this need nor knows what the psychologist can offer.

McDanald: Nearly all physicians are wanting for information as to what the psychologist has to offer. Since we are discussing chronic disease, the psychologist needs to point up how he can assist any physician in spotting emotional factors which may be contributing or complicating factors in chronic disease. Hence the initial step is to have the psychology department define itself for the benefit of other departments.

Kubie: This comes back to a plan for a *Department of Behavior* which would unite men with clinical interests more closely to men of psychology, and would bring back into the university fold. Such a department would provide a framework in which to reconcile research needs, teaching needs, and service needs, one in which the psychologist could function better than in any other framework we could devise. Thus, should those who acquire their additional clinical skills have an additional doctorate of any kind? In what would this training consist? Do we have to consider what the premedical, prebehavioral experiences in college of the candidates for admission should be?

Brown: This might provide the clinical acceptance for a form of internship for the clinical psychologist.

Kubie: Let us think of the start of their training in such a department, a Division of Behavior, which would be equivalent to admission to medical school. They would come in as college graduates with Bachelor's degrees.

Berry: You go to college to gain an educational experience. It should equip you to go on in any direction, not just in the area in which you have majored.

Holsopple: With respect to the designation of the area in which we are interested we must recognize that the profession of "Psychology" as such offers some satisfactions, not because they are in *behavioral sciences*, but because they are in "Psychology." If we are to set up such a program as this, we want our very best psychologists in it. We want it to be in the behavioral sciences, but we also want it to attract our good psychologists.

Kubie: In this program, I would emphasize exact quantitative fields. There is something diffuse in the study of psychology, and

unless a man enters this field with a fair grounding in exact sciences, he may never get them.

Jacobsen: I would like to see anyone going into psychology have a background in physics, chemistry, biology, etc., equivalent to minimal standards set for admission to medical school.

Kubie: Roughly what we have said is that the emphasis is on not the specific content of the college curriculum, but on the quality of a man's work, plus those minimal science requirements which currently are the minimal requirements for admission to medical school.

Ham: This Department of Behavior, then, would be like a basic science department in medical school?

Kubie: We might conceive of the organization of the curriculum as comparable to that of any basic science department of a Department of Behavior in a medical school, as the foundation on which the whole clinical psychology and psychiatric department would base both its applied work and its research work. My picture here is of a good basic Department of Behavior, more closely allied to the graduate Department of Psychology than usually is true. You said that all medical students would be instructed from this department; but would all of those enrolled in the Department of Behavior go through the full medical curriculum?

Jacobsen: This would not be necessary.

Kubie: What are the steps in the curriculum offered in a Department of Behavior?

Ham: Could one think of this as another department? Do students get this in addition to other things, or are we dealing with a basic type of training applied to all persons dealing with people? Then do they make a decision to be surgeons, psychologists, etc.? Are there two groups only involved—one behaviorists and the other clinicians?

Kubie: This would be offered to all medical students, and presumably to students from the university as well; but in addition a group of students would not take the entire medical curriculum, just as there would be medical students who would not take the entire curriculum in the Department of Behavior.

Berry: They would decide whether to go on with medicine or other graduate work. The physicians would go along to a certain

point and there branch off. There should be a central component for all who come into school to work in this basic area.

Ham: Then there would be some who might decide they would want to be Ph.Ds. in biochemistry or physiology, and others who would become physicians. They would not be involved later as research workers in human behavior. Others would go on with such research, however.

Kubie: There would be some going on to become physicians, some Ph.Ds. in psychology for research, and some going on to doctorates in medical psychology. This Department of Behavior could conceivably become as big as the whole medical school. Those who remain in various departments for Ph.D. work are always small compared to the medical student groups.

Jacobsen: Another aspect is that if some of these people wanted to go on into slightly different areas, they might move over into the Department of Sociology or Psychology, in the university.

Ham: Having gotten this matter of a central group of students and then the choice of going on with a Ph.D. or clinical application, there has been talk also of going on and being psychiatrists. That brings up the problem of the development of a doctor of medical psychology and also the development of psychologists. This leads to the problem of first- and second-rate citizens, so to speak.

Berry: Those primarily interested in therapeutic aspects could become counselling psychologists; those interested in educational and research aspects could find academic placement.

Kubie: Can one embrace in such a Department of Behavior any of the ancillary disciplines?

Berry: In such a department, members of the medical school faculty could be members simultaneously of the faculty of the College of Arts and Sciences.

Ham: At North Carolina, we have an institute within the university which is in a department but has in it psychology and medicine. The group functions together and out of the institute grow research projects.

Holsopple: Here, though, we are talking about something different from what we originally proposed. These institutes are set up and really have no home other than the graduate school.

What we are proposing here is a Division of Behavioral Sciences within the medical school.

Kubie: None of us has the thought that we have a perfect solution. This is just a starting point in our effort to find the type of solution which could be modified to meet local situations; a kind of approach to the problem which might be useful. We then should go on to consider steps in the evaluation of the curriculum.

McDanald: What are the alternatives?

Kubie: To review in detail what is actually happening in different places.

Fremont-Smith: Are there any clear developments that need to be introduced in the majority of places where they do not now exist? Are there large areas where there is almost no insight such as can be provided by the clinical psychologist? If so, can we take lessons from places where it is going well today and make a recommendation? Can we say anything about the training of physicians and other adjutant people in hospitals that have to deal with large bodies of chronically ill?

Holsopple: We are interested here in chronic illness. It is true of medical students and is unfortunately also getting to be true of psychologists that they come into hospitals and clinics and are happy only so long as they have interesting people to deal with. They lose interest when they get to the chronic patient. Our training program should prevent this.

Watson: We are at an information-sharing conference. I would welcome the opportunity to share certain thoughts on ways of meeting the existing problem with current resources. This idea of a Department of Behavioral Science is something we can use.

Kubie: We are making a break to the question of what can be done *now* to stimulate interest and to train current existing personnel of all kinds so as to increase their interest in and skill with the chronic patients.

Ham: At North Carolina, the Dean has set up a Rehabilitation Committee. On it appears surgeons, internists, pediatricians, psychiatrists, psychologists, social workers, phyical therapists, etc. The goal of this program is to exchange information which we all know is available today with our present training. It is being brought to bear in how best can we deal with this problem as part

of our general operation. Do we want to have a separate area for this or not?

Watson: There are two or three ways I would like to suggest by which we might implement the work of the psychologist in chronic illness. Several have mentioned that rehabilitation is a much broader issue than merely within the medical school and the hospital. Psychologists are employed in many other agencies. I would suggest then the consideration by the administrators of medical schools of appointment to the medical school staff of clinical psychologists in the narrowest sense of "clinical," meaning a part-time appointment. This has been done at certain medical schools. We could implement this through appointment of psychologists from the rehabilitation agencies of the community. This would be one way of bringing within the framework of chronic illness and work in that field those who are not at present connected with medical institutions. Secondly, I would suggest that serious inquiry be made about the possibility of setting up more training stipends at teaching hospitals for work of the psychologists. Third, I would give an affirmation of faith that the training of the clinical psychologist should have more emphasis on medical work, including acquaintance with the chronically ill. At the present time most psychologists do not like to work with them. We must come to see the challenge of this field of appreciation. At the moment, there is nothing to make it attractive for a psychologist to work with the chronically ill. Some introduction of material concerning graduate training possibilities in this area should be brought to the attention of the psychologists in the universities.

Holsopple: Our V.A. patients are predominantly the chronically ill. They are viewed as such. It is not always easy to distinguish between therapy and research in dealing with the chronic illness. I think we can accelerate interest in the chronically ill with the people we now have.

Kubie: There is general agreement that the concern with the chronically ill on the part of the internist, social worker, psychologist, psychiatrist will have to be stimulated by integration of this with a research program and by showing that the chronic is often more malleable than we imagine when he is left to stew in his own chronicity. Although there are stipends and fellow-

ships available for some preparation for work in this field, perhaps there is not enough specifically designated or not enough of this.

Jacobsen: At the Polio Board meeting, we set up some fellowships in rehabilitation. This got tied up with a provision that these should be carried out in units with a competent psychiatrist to supervise the work. Was it proper to have this so constricted?

Brown: Having been dealing with chronically ill patients for a number of years, I feel that before the setting is complete for psychologists to enter the picture there must be an atmosphere of professional interest in chronic illness on the part of the medical personnel. That begins with medical students, interns and residents. Previously, in our case, interest was lost after the initial work-up and discouraging statements were made at the bedside. In our program of reorganization of a medical department we began with an alteration in the attitude of our medical students as in their approach to the patient. We pointed out the therapeutic value of properly conducted rounds and finally we have developed sufficient interest in this to make chronic disease even more interesting and appealing than a number of acute illnesses that rapidly come and go. We have finally developed an atmosphere into which we can inject any chronic problem without loss of interest on the part of the physician. We have also developed a department which we term Medical Integration and Rehabilitation. An internist heads this group, with all the sub-specialty services represented in support of the patient problems. I would also re-enforce the essential union between the research and management of chronic disease in maintaining vitality and interest in these problems.

McDanald: You said one of the objectives should be integration of the several disciplines on the basis of research. Such an approach may be entirely practical, but if I may speak concerning our local need, I believe that first we should make arrangements for education by the psychologist of the other departments as to what services could be rendered in connection with their problems. In the educational process we can talk about what needs to be done towards acquainting departments as to how the psychologist can be used in the framework of the therapeutic team.

Harrower: Every research project can be a therapeutic project so far as the patient is concerned. Why not have a research project out of which grows an educational project?

Brown: You can teach in a progressive, sustained fashion only through research in my opinion. Good clinical research is a necessary part of management of chronic disease.

Ham: The danger of trying to "teach" heads of departments about what you have to offer is a pitfall in that sense. The people who have these various disciplines are working together and the values show through research and service. Medical schools and hospitals now are encouraging highly trained people in the various disciplines.

Watson: The best way to encourage research collaboration is to encourage individuals to ask psychologists questions about their own research. The psychologist in a medical center should remember that he must be eager for an opportunity to follow through from such discussions and be receptive to helping his colleagues on their own particular problems.

Kubie: It is the consensus of feeling that the focus on the chronic patient involves a closer relationship of the acute and the chronic problem for the enrichment of each in terms of the clinical psychologist, medical student, house officer and psychiatrist. How do we get in there to do the job? If the Department of Psychiatry in a medical school holds itself aloof from medical problems, the psychologist stands on the outside of this. If he tries to establish a direct relationship, the fur will fly indeed. If the Department of Psychiatry is working with the other departments, then the clinical psychologist will be part of the team, too. If you do not have those particular obstacles, how can the psychologist or psychiatrist exercise an informative influence? I am sure it is through being part of a research team. This has more effect than any lecturing. It is a demonstration of what one can and cannot throw light on. This means not merely research on therapy, but research on basic processes. The investigation which starts without bias usually encounters the least resistance and can accomplish the most when it comes up with results.

Brown: In our research team, we have selected people who can also handle patients at the most highly qualified level and who know when to ask for help. The good internist is particularly

valuable because he knows his limitation in any field. You must have personalized interest in the patient. In our program the clinical psychologist would integrate principally with the key internist. What is the opinion here? Should the psychologist integrate through the psychiatrist, internist, etc., or through the educational structure?

Berry: All of these have possibilities. Grab anyone, whenever you get a chance! The secret is the community effort of the group —all focused on the patient—all trying to do something about it. It should all relate broadly to medicine.

Kubie: The ordinary hospital psychiatrist knows nothing about the problem of the neuroses. Psychiatrists dealing with the neuroses want to work in this field, but very few have hospital or medical school appointments. Also, they are belabored by demands for help by human beings. We must have people trained to deal with the neuroses and we must have them in sufficient numbers.

Ham: The pressure for service makes research more difficult in psychiatry than it should be. There is no question but what all of these disciplines together have a great deal to offer. The best way for teaching is through actual working together on a problem. In this way those things which the psychologist can bring to medicine will be brought forth. Will not this help bring a solution to the problem by working together?

Brown: Economics is often a limiting factor.

Kubie: Part of the heat in this whole problem of the relation of the clinical psychologist to the psychiatrist arises out of the fact that the clinical psychologist often knows a lot about neuroses. Yet if he is put into a formal organization he finds there a psychiatrist who may know less about neuroses than the psychologist and then the psychologist is unhappy.

Holsopple: The tendency for this to happen is less than it was five years ago. Lack of whole-hearted support for the psychologist who does know what he is doing is often discouraging.

Ham: At our institution, the question of who would put money in the budget for clinical psychologists came up. The idea was to demonstrate to other areas that it is worth having money in the budget to pay for services of these people. This comes back to the statement that the situation is helped by demonstration of service.

Brown: We need more and more emphasis on medical people obtaining psychiatric training to bridge the gap. All of our medical internes and residents have some psychiatric training which helps a great deal.

Kubie: In closing it must be clear to all that the training program for medical and psychological teamwork in handling the chronically ill offers a challenging and complex problem, from which individual biases must be eliminated by the test of practices which may help best to achieve the goal of as much rehabilitation as possible for the patient.

DISCUSSION OF REPORT OF STUDY GROUP ON TEACHING AND TRAINING

Fremont-Smith: One would not anticipate that there would necessarily be only one right way of relating psychologists problems with health problems. It might aid to consider various ways in use throughout the country. Nurses, social workers, physical-therapists et cetera, might play a different part in a large university, whereas in a sparsely-settled part of the country they would have to play the role of psychologist where there wasn't a psychologist or psychiatrist available.

Waston: It was proposed by one of the members of the group that the day will come when a department of behavior will be organized in a medical school—a department that will be more comprehensive than the discipline of psychology.

Kubie: There was a good deal of pro and con on that question. Some of us feel that by bringing in too many ancillary people, one might create enough rivalry to destroy the effectiveness sought.

Watson: This was discussed in a variety of settings such as experimental psychology, clinical therapy, et cetera, but here we are more interested in problems at the levels of research and teaching. There are these larger issues quite apart from our relationship to the chronically ill. There was some mention of the independent practice of clinical psychology, but not in relation to caring for the chronically ill.

Seidenfeld: Would you qualify a little further the concept of the development of a department of behavior in a modern university with a medical training program? Would it be a department differing from the departments now ordinarily existing in the fields of psychology, anthropology or sociology?

Jacobsen: The proposed department of human behavior was thought of as building a bridge between various psychological interests in a university. Such a department might well have responsibilities in terms of teaching medical students. It would be set up primarily for co-ordinating teaching and research in human conduct. It might be a division of the department of psychology, or of the department of sociology. Where there is a good relationship

in a university, one wouldn't notice a line of demarcation in these fields. In some universities, there might be nothing as a base for considering human relations. Here a special department of behavior might be in order. Such a field would correlate with clinical psychology, sociology, cultural anthropology, physiology, and even ethics.

Seidenfeld: I feel a survey might be made to the advantage of all concerned as to where clinical psychologists are actually being used in the care of the chronically ill. This might point up the problem in regard to institutional care of the chronically ill.

Jacobsen: There are at least two surveys of how psychologists are working in a medical setting: one made about 1948, and the other currently being prepared by the Medical School of Washington University, St. Louis. Some information on this matter might also be found in reports from the Committee on Education of the American Psychiatric Association.

Lee: Should the content of training of clinical psychologists for teamwork in caring for the chronically ill be supervised by the medical faculty in medical schools?

Watson: By a cooperative interdisciplinary program this might be worked out. At the present time some 25 medical schools do offer some training in clinical psychology at the graduate level, and many also offer opportunities for supervised clinical practice at post-doctoral levels. Training in clinical psychology is also offered as part of the curriculum for medical students.

Jacobsen: I think responsibility for the doctorate in clinical psychology should be entirely on a graduate level apart from the medical curriculum, but might be worked with some relation to medical training in neurology and psychiatry. I think the advantage of having a stronger tie with medicine, with a section within a department of psychiatry or behavior, would be to give some responsibility to medical teachers in training clinical psychologists which is now lacking. I think it could improve the preparation and quality of clinical psychologists for care of the sick.

Holbrook: Aren't we neglecting one of the most important groups requiring orientation in psychological matters in chronic illness? I mean the bulk of medical practitioners, general practitioners and specialists. What may be achieved in improving attitudinal relationships between those who care for the chronically ill and the sick patients themselves will depend basically on the orientation of the great majority of the physicians that are going to take care of these patients. What are we going to try to do toward the teaching and training of some 180,000 physicians who are actually now in practice with the problem of increasing numbers of chronically ill, and who are essential in the medical and psychological team if it is going to function at all well in caring for these chronically sick people? Perhaps a primer on psychological factors in chronic illness could be prepared for nation-wide distribution to practicing physicians.

Kubie: We cannot seriously maintain that it is a practically attainable goal to alter basically the attitudes of the majority of practicing physicians towards psychological factors in the care of chronically sick patients. Those whose own experiences in living and in practice were bringing some enlightenment or

insight without any pressure from us will reach out for psychological aid. We thought it wise to concentrate on training medical students to realize the importance of clinical psychology and the help it may bring in handling sick people.

In medical schools, the creation of a department which recognizes human behavior as a basic science might be a long step forward toward integrating training in clinical medicine, psychiatry and clinical psychology. Such a department might be charged with training medical students and other auxiliary medical personnel in teamwork with clinical psychologists in the care of the sick. There is little point in trying to spell out the curriculum for such training at this time. It might be worthwhile, however, to indicate that such a training program would help to provide adequate care for sick people.

In any such proposal, it may happen that psychiatrists may appear to be the wall between internists (or various medical specialists) and clinical psychologists. Sometimes where good psychological personnel is available, liaison with internists or other physicians is difficult because psychiatrists stand between. Medical schools might aid greatly in promoting medical and psychological teamwork in the care of the sick by developing practical methods of psychiatric participation and cooperation.

Kohn: We must not underestimate the capacity of the physician at large to understand and to use whatever may be offered to him which may aid in caring for his sick patients. I have been impressed with the type of post-graduate work of the Regional Hospital Council. Under current post-graduate medical efforts, the average doctor is often way ahead of where we assume him to be. Usually he complains not that he is pushed too fast by new ideas, but that not enough are given him. We should use all possible methods to make clear to practicing physicians everywhere what clinical psychology can offer in caring for the sick. Doctor Holbrook's idea of a primer on psychological principles in the care of the chronically ill would be well received by doctors and by the public as well.

Leake: It woud seem wise to help promote interdisciplinary contacts wherever possible in the health fields. In showing what clinical psychology may do in helping to take good care of the chronically ill, advantage should be taken of such periodicals as *GP*, the *Journal of the American Medical Association*, and state medical journals, not only in illustrative articles, but editorially also.

Ham: Leading medical schools today are beginning to introduce psychological concepts early in the medical students' training. The idea of a department of human behavior in a medical school is a welcome one. If developed well, such a department could accomplish much in integrating important clinical and psychological training which might otherwise be quite haphazard. The idea of trying to teach something about clinical psychology to practicing physicians might call for interesting attempts at new kinds of refresher courses, to which clinical psychologists themselves might contribute. Doctor Harrower, Doctor Seidenfeld, Doctor Garrett, and many of their associates have indicated how such post-graduate medical training in utilizing clinical psychology might be developed. But clinical psychologists are afraid of over-

selling their contributions to medical care. They have developed excellent techniques for testing and for aid in treatment, but overselling may boomerang. They feel it wise to go slowly, so that clinicians may really understand what they can do to help in caring for the sick and thus welcome them into team-work cooperation.

Holsopple: The overlapping area of psychology and the other behavior sciences within the medical school could constitute a unit of the medical school curriculum and at the same time a unit in the basic sciences. The student within that unit, if he were going into medicine, would be bound by all the rules of the medical school curriculum. If he were going toward a Ph.D. in psychology or a related discipline and were physically within the medical school unit, he would be guided by the curriculum requirements of the graduate school. The faculty member would have a dual appointment as a member of the medical school faculty and as a member of the graduate school faculty as well. It would not be necessary for the student to make a choice of M.D. or Ph.D. goals until he had acquired the information upon which he could make an intelligent choice.

Lee: Such a dual program might also be applied toward education of general practitioners in medical and psychological teamwork. About this matter of salesmanship—don't underestimate the capacity of the general practitioner to appreciate these things. It is extraordinary in its significance to note how quickly doctors seize upon improvements in practice. Many items have been considered in medical and psychological teamwork in caring for the chronically ill, but we are in no position to offer a finished program. Many steps are needed before we can propose a working procedure. Much research is necessary and then validation of procedure. Programmed practice may be recommended and developed from such research and validation.

Cobb: I would like to emphasize one thing Dr. Ham said concerning doctor training. In working with cancer patients from day to day, I have learned the devastating results of counter-transference on the part of the doctors. It isn't what a doctor actually says or does that counts but what the patient thinks he does. When a doctor hurries, the patient often interprets his haste as rejection or dislike. When a doctor speaks abruptly or absents himself from the patient's room, the patient reads into these actions many things. After all, the doctor is only human and it is difficult for him to like all of his patients, perhaps it is impossible for him to do so, but it is important for him to realize that he doesn't like the patient and to have sufficient insight to handle the situation in such a way that the patient will not suffer needlessly. Medical students need help in recognizing, evaluating and coping with this powerful factor known as counter-transference and I am delighted to learn that people of Dr. Ham's position and efficiency can work with them on this problem.

Rottersman: For several years I have had some responsibility in the organization and conduct of post-graduate courses in psychological medicine for the general practitioner, and I am well aware of the fact that practicing physicians are desirous of information on any new idea or procedure which may prove helpful in the care of their patients. I know that they will welcome whatever

enlightenment can be given them as to the ways and means by which clinical psychologists can be of aid to them in their medical practice.

Erik Wright: It takes time to reach effective communication between people in different fields of endeavor. We should realize that it will take lots of time to get practicing physicians to know what clinical psychologists may do to help them in caring for the chronically ill, even if there were enough clinical psychologists available to do the job. But we should get started. I agree with Doctor Rottersman that practicing physicians are hungry for new clinical ideas in handling patients. County medical society meetings might be fruitful places in which to get physicians and clinical psychologists together. It may be just as important to get persons together as to distribute ideas.

Leake: Overselling may have its compensations in calling attention to defects which may be correctable. Co-operation between medicine and clinical psychology may require much time to develop smoothly. But the effort will have to be made out where the doctors practice. Post-graduate training courses in medical use of clinical psychology would attract few physicians to training centers. Short programs with frequent development at county medical society meetings would be more effective.

Fremont-Smith: It remains important to get this awareness of cooperation between clinicians and psychologists across to medical students. This may then be expanded to include social workers, nurses, physical and occupational therapists, and even hopsital administrators. This would really aid the chronically ill.

CHAPTER V

THE PROS AND CONS OF A NEW PROFESSION: A DOCTORATE IN MEDICAL PSYCHOLOGY

LAWRENCE S. KUBIE

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I. TO ELIMINATE MISUNDERSTANDINGS

In this matter there are several partisan attitudes which others defend but in which I do not believe. Therefore, I must begin by spelling out the various extreme positions which have been attributed to me, but which I would disown. In these disclaimers I deviate from the more or less official positions which at various times have been taken by the American Psychiatric Association and/or the American Psychoanalytic Association and/or the American Medical Association, or various executive organs or subcommittees of these scientific bodies. (There is no need to be precise or pedantic about listing these.) At the same time I also deviate from the over-confident position taken by many clinical psychologists. I stress this because from time to time sentences

or paragraphs from what I have said or written have been quoted out of context, and in some instances grossly misquoted, making it apppear that I am an uncritical and biased partisan of the claims of the extremists among clinical psychologists. We can understand how this occurs in the heat of battle; and it is easy to forgive, whether it is done by those with whom I tend to agree or by those with whom I tend to disagree. Unfortunately, however, as a result of these out-of-context quotations, many colleagues and critics assume that I occupy a more extreme position than actually is mine. This makes it difficult to win a dispassionate and objective hearing for my essentially moderate and realistic plan. It is for this reason that I make this series of explicit disclaimers, in the hope that what I thereafter advocate will be considered on its merits and with an open mind. Indeed, this is the only justification for interjecting so personal a note.

Specifically, these are some of the things which I do *not* believe. I do not believe that the clinical psychologist, trained as he usually is today, is competent to make independent psychodiagnostic judgments or to undertake psychotherapy, whether under supervision or independently. I state this at the risk of alienating many friends among clinical psychologists, and despite the fact that I am fully aware of many exceptions. There are clinical psychologists who have as keen a psychodiagnostic judgment as any fully trained psychiatrist or psychoanalyst. There are some clinical psychologists and some non-medical psychoanalysts who are among the subtlest and most effective psychotherapists that I have ever known or expect to know. These, however, are not the rule. Furthermore, these exceptions exist not merely because of their special psychological aptitudes, but rather because of the content and quality of their training, the length of time they have invested in that training, their prolonged exposure to contact with human beings who are sick physically as well as mentally, and finally the many roles which, partly by good fortune and partly by design, they have been privileged to play in relation to patients with a wide variety of organic and psychological ailments.

It is this richly diversified experience which has helped to make them into diagnosticians and therapists. Yet the fact that such exceptions can and do exist does not prove that the average

training of the run-of-the-mill clinical psychologist meets the requirements of diagnosis and therapy. The exceptions prove only that it is *possible* to train behavioral scientists to become competent and mature in psychodiagnosis and psychotherapy, without complete medical training or medical licensure. They prove that this is scientifically possible. Whether it is safe, and under what circumstances, whether it is *necessary*, whether it can be made *practical* on a wide scale, and therefore whether this should become our general goal are issues that will be discussed below.

There are those in the field of medicine and specifically in psychiatry who wish to exclude from participation in psychodiagnosis and psychotherapy anyone who does not have a full medical training, irrespective of what other type of scientific training he has mastered. There are other members of the medical and psychiatric professions who indicate that they have no objections to allowing non-medical presonnel to make diagnoses and to conduct therapy, provided that they practice only in medical institutions and under close and continuous supervision. I want to dissociate myself from both of these positions. My personal belief is that non-medical behavioral scientists can be so fully trained that it is safe to allow them to carry on psychodiagnostic and psychotherapeutic activities without psychiatric supervision, provided that they practice under legally prescribed conditions of cooperation with physicians, which will safeguard their patients against the risk that co-existent or intercurrent organic disease or psychosis will be overlooked. I believe, furthermore, that these safeguards can be made quite as secure as when a medical psychiatrist conducts psychodiagnostic investigations and subsequent psychotherapy. My proposal is based on a conviction that such safeguards can be established first through processes of selection and training which will be outlined below, and second by establishing legal conditions of licensure and practice which will also be described below. This implies my further conviction that these safeguards should be spelled out in detail, first in the curriculum by which men are trained and secondly in the laws under which they practice their profession. On the other hand, I do not believe that the welfare of the individual patient, or the urgent needs of the community, or the best interests of scientific progress would be met if we were to require that fully

trained psychologists, merely because they are not physicians, must conduct the processes of psychological diagnosis and psychological therapy under a system of moment-to-moment supervisory checks. On the contrary, I believe that such a requirement would defeat our aims completely, and that other and better safeguards can be set up, which will at the same time enable us to attain our goals.

I want next to disclaim the word "lay," in such expressions as "lay psychologist," "lay therapist," "lay analyst." If by "lay" one means someone who is *without scientific training*, then I do not believe that any such *layman* has a contribution to make in any field of diagnosis or therapy.[1]

If the word "lay" connotes a lack of familiarity with the basic facts of human biology in health and illness, or if by "lay" one means someone who had had no opportunity to experience how human beings behave when they are physically sick, or if by "lay" is meant a scientist who has never had an opportunity to steep himself in the mores of the best medical traditions of therapeutic responsibility and therapeutic skepticism, then for "laymen" in any of these senses I can see no role in either psychodiagnosis or psychotherapy.

My positive position is that the minimal basic training must consist of scientific methodology, the use of statistical devices, the basic exact and biological sciences, plus specific training in the behavioral sciences. I believe that these constitute the essential foundations, but that alone even this is not enough. To them

[1] For this particular confusion over the concept of a layman, there is an ancient history: the story of healing cultists of all varieties, the bone-setters and osteopaths and chiropractors, the bubbling spas and chemical springs and healing mudpacks, the story of homeopathy, the search for Fountains of Eternal Youth, waters to make us invulnerable to arrows and swords, Paracelsus and Mesmer and non-medical hypnosis, the fantasies of autointoxication to be relieved by red, white and blue enemas, both high and low, all of these are part of this history.

In more recent times, Freud himself set us a confusing precedent, not merely because the lay analysts whom he trained and defended were not physicians, but rather because a number of the more vocal among them were wholly unscientific in training, and in their lack of understanding of the essential processes of scientific evidence. Some of these brought to their analytic work a great imaginative flare, equalled only by their scientific naivete and arrogance. As with the philosophers of old, the intensity of their feelings was for them the only criterion of truth. In certain specific ways analytic theory suffers from this precedent even to this day; because many medically trained analysts accept this bad example as an adequate model of analytic reasoning and evidence.

must be added: (a) human biology; (b) human pathophysiology; (c) the general psychology of organic and psychological illness; (d) the highest medical traditions of therapeutic responsibility; (e) the tradition of unsparing skepticism and self-critique about naive therapeutic claims and assumptions; and (f) the specific techniques of psychodiagnosis and psychotherapy. . . . All of this must be incorporated into the fibre of the psychologist on his path toward his new profession: and I believe that this can be acquired only under the auspices of medical schools and teaching hospitals. Therefore, as I look ahead I do not envisage in this field any unscientific laymen, nor even good scientists who have had no exposure to medical data, medical procedures, medical ethics, or the climate of therapeutic skepticism which centuries of defeated hopes have bred into the physician. The non-medical scientist whom I envisage, as standing shoulder to shoulder on an equal plane with the medical psychiatrist, must have had prolonged and intimate exposure to all such experiences.

I disclaim also the popular, misleading and naive idea that one can make a sharp distinction between health and sickness, between normality and neurosis, or for that matter between neurosis and psychosis. Consequently, I dissociate myself absolutely from the idea that it is possible to make a rigid distinction between simple and complicated forms or levels of psychotherapy. Limited psychotherapies (such as vocational counselling, speech training, remedial reading, family counselling) do indeed present differences in techniques. Yet any human being who needs such help will at the same time be struggling with problems arising out of a concurrent neurotic process. Therefore, retraining, re-education, testing and counselling can rarely achieve their goals unless the coexisting and interrelated neurosis is dealt with at the same time. Realistic therapy usually involves a comprehensive attack; and although technicians in these various special fields can assist the fully trained psychotherapist, they can never work effectively if they work alone. When they are set up independently in reading clinics and speech clinics or vocational advisory clinics or aptitude testing clinics, they soon find themselves paralysed for lack of adequate data on the total psychopathological soil out of which the specific disability has grown

and to which in turn it has contributed. Consequently when I propose a full curriculum of training in psychodiagnosis and psychotherapy for scientists who are not physicians, I do not have it as my goal merely to throw to the non-medical therapist small crumbs of remedial reading and vocational or other counselling. I envisage someone who will be equipped to meet the total challenge of psychodiagnosis and of psychotherapy quite as skillfully and as maturely as it can be met by the best psychiatrist at the present point in the evolution of our knowledge of psychopathological techniques. If we are to attempt to develop this new profession, this must be our goal and this must be the minimal standard against which we measure our success.

Finally, I want to disclaim any idea that I have a solution which will break the existing bottleneck *at once*. My purpose in presenting this plan is to place before you a long-term view of the problem and a long-term solution, and to emphasize above all the fact that the solution must start with the *training of a corps of teachers*. This must be done before the definitive training of practitioners can be undertaken, if these are to achieve the level of competence at which we aim.

In other words, I propose a new para-medical discipline, a doctorate in medical psychology, trained in medical schools and teaching hospitals jointly with academic university departments, in a coordinated curriculum compounded of all of those disciplines which are important to an understanding of the mind and heart of man, both in illness and health. How teachers are to be trained, how students are to be selected, how the curriculum is to be set up, how rapidly it will increase the supply of competent practitioners, and how they are to be legally established in actual practice, are topics which I will discuss below. Let me only point out that this is no "quickie" tour that I propose. It will require several years of graduate study.

This is a long introduction, a long denial, a long exposition of what I do and do not believe. I hope it will clarify what is to follow.

II. A Philosophical Foundation

In this paper my major emphasis will be on certain pressing social, medical, and psychological needs; and on the practical

ways of meeting these needs. Yet I do not want to give the impression that these are my only reasons for being concerned with the problem. Actually my interest has broader cultural implications, which are at least as important as are the immediate requirements of the mental health of the community; although in the end the practical and the cultural goals are interdependent.

Psychoanalytic psychiatry is making possible a profound social and cultural revolution. It has long been acknowledged that man has been able to change everything in his world except himself; and that all of his learning about the outside world has not changed him. In recent decades psychoanalytic psychiatry has given him a tool which for the first time in human history makes such change possible. Hitherto the human race has not been up to the business of being human. Perhaps if we learn to use this new tool, we may become able to meet this challenge. Specifically this will mean shifting the balance of power in human personality out of the hands of the destructive alliance between unconscious and preconscious forces, and into the hands of the creative alliance between conscious and preconscious forces (19).

But if we are ever to achieve this, mankind must profit by the lessons which can be learned only through the study of psychological illness and failure. In psychology as in all of medicine, illness is the ultimate teacher. Consequently, every phase of culture must be infused with the knowledge of health that comes from an understanding of illness. This is the creative significance of the current preoccupation with illness, which characterizes modern art and modern literature, confused and uninstructed though this remains today. The same principle applies to sociology, anthropology, economics and government, to the organization and mores of the family, to such unsolved problems as marriage and divorce, to the methods by which we bring up our children, and indeed to all of education. As I have pointed out elsewhere (18), self-knowledge in depth is the forgotten man of education; and because it is forgotten, the failure of our culture is measured by the fact that no man has been able to transmit to succeeding generations whatever of wisdom he has gained.

Thus there are disciplines which provide man with techniques for living more effectively, while he himself remains spiritually unaltered. There are disciplines which give men techniques for

understanding and even in some measure for controlling the external forces of nature. There are disciplines which give men inspiration, consolation, or many-sided definitions of the good life. There are disciplines which give men an understanding of the techniques of organized society and of the history of man's effort to live in social groups. To everyone of these must be added an infusion of knowledge from the one discipline which deals directly with the nature of human nature and those involuntary distortions called neuroses. From this point of view, the goal of culture itself is to set man free from the imprisoning tyranny of his own unconscious, free that is to achieve the capacity to change.

It is my contention that the direct and first-hand knowledge of the neuroses which grows out of the experience of dealing with them is an essential ingredient of any culture worthy of the name, and that such knowledge cannot become part of man's general cultural heritage, if it remains the exclusive domain of medicine.

It is for this reason that the knowledge which comes from the clinic and the lessons learned from pathology must inform every aspect of our cultural and educational systems. This includes the factual data of illness, detailed familiarity with the varied manifestations of psychopathology, the diagnostic and therapeutic implications of that knowledge, and an understanding of the processes of falling ill and of falling well. By bringing these hard facts out of the seclusion of the clinic into general currency, clinical psychoanalytic knowledge will make a contribution to every allied discipline: to wit, to cultural anthropology, social psychology, experimental animal psychology, ethology, clinical psychology, mythology, education, philology, the law, penology, and theology.

If, however, psychopathology is to inform these other disciplines, then it must draw into itself some of their practitioners. It is only by growing up side by side, by working shoulder to shoulder with them, that an interchange can be achieved which will give us a sophistication about their fields and which will give them a sophistication about psychiatry to carry back into their own disciplines.

This is particularly true for education, where it is impossible to make a sharp differentiation between the processes of educa-

tion and the processes by which we develop neuroses and health. Indeed, the immediate health of the generations which are hard on our heels depends upon a rapid integration of psychiatric knowledge into the stream of our educational system.

The tangled interweaving of the processes of learning with the neurotic process gives support to my general argument. In this close interdependence there are two main factors. There is first the role of repetition in both. By imperceptible gradation the repetitive drill of the learning process shades into the automatic and involuntary repetitions of the neurosis; and as a consequence of this contamination of the learning process by neurotic automaticity, repetitive drill makes imperfect quite as frequently as it makes perfect. This distortion of the learning process by neurotic repetitive mechanisms often frustrates the practice of the athlete, musician, the student of languages, or indeed of any effort to memorize anything. It results instead in the formation of neurotic work-blocks as a form of neurotic defense; and every clinical psychiatrist knows that such blocks frequently defeat the educational process completely.

A second difficulty in learning procedures arises through a neurotic distortion of the symbolic process. The symbolic process always represents a condensation of conscious, preconscious and unconscious symbolic values: and in elementary learning, as in the highest levels of research (16) there can be a subtle and unnoted shift from the conscious import of anything to its unconscious connotations. This in turn can initiate blindly compulsive workdrives in unprofitable directions. One encounters this repeatedly in the lives of scientists, artists, writers, etc. Alternatively this may set up equally blind resistances to the processes of learning, resistances which may be either selective or all-embracing. In this way the symbolic process can become an obstacle to the learning process instead of its chief tool.

Thus at every level of education, from the kindergarten to the highest echelons of post-graduate study, educational procedures become intricate networks of normal and neurotic mechanisms; and because neurosis and learning employ similar mechanisms, in the learning process there is also an inherent trend towards distortion, the correction of which demands a continuous use of appropriate technical preventive and therapeutic devices. People talk glibly of neuroses as a kind of mal-education and of psycho-

therapy as a learning process. Both statements are naive oversimplications, which overlook the fact that education itself makes use of the essential ingredients of the neurotic process, with the result that one cannot dissociate the learning processes of education from the understanding and control of neurotic mechanisms. Therapy and learning constitute together a joint field for education and psychiatry. Therefore, education and therapy are both complementary and inseparable. This is why even alone the challenge of education should blast us out of any smug notion that psychodiagnosis and psychotherapy can be reserved for the psychiatrist.

This then is the philosophical basis for my dedication to this cause. Just as formal psychiatry left the "lunatic asylum" to enter the community, so we now must face the fact that psychopathology as a discipline, as a theoretical system, as a field of human understanding and as a technique of exploration and of therapy must escape from its confinement in the medical clinic, to offer its knowledge and its skills to all of human culture. Apart from the treatment needs of the community this is the cultural voyage upon which we are now engaged, and to which this paper is dedicated.

III. The Logistics of the Problem

A. The Patient

The most recent survey of the dimensions of the challenge issued from the office of Mrs. Mary Lasker in the spring of 1954, to cover the year 1953. These statistics were gathered from all available sources. They are concerned primarily with the nine million psychoses (more than one in every 17 persons) in the United States; plus the 2,400,000 mentally subnormal individuals, ranging in age from infancy through adult life; 1,750,000 serious crimes; 50,000 narcotic addicts; 3,800,000 problem drinkers, of whom 950,000 are chronic alcoholics; 17,000 suicides; one divorce for every four marriages; and the 265,000 children between the ages of 7 and 17 brought to juvenile courts. What this means in terms of suffering, human loss, loss of military manpower, loss of defense production, civilian economic loss, plus the burden of psychiatric care is self-evident.

With respect to this staggering problem, I will omit any consideration of problems of research, or of hospital building and administration, to concentrate on the question of personnel, since this alone is relevant to my thesis.

To serve these needs we have 7,608 psychiatrists who are members of the American Psychiatric Association; but of these more than 2,000 are administrators, superintendents and commissioners. There are only 4,348 diplomates of the American Board of Psychiatry and Neurology, some of whom are not yet members of the APA. Estimates as to our needs for psychiatrists to deal only with these problems vary between 10,000 and 20,000. It is estimated, however, that once we attempt to deal with the problem of the neuroses as well, we will need two or three times as many people, trained in psychodiagnosis and psychotherapy. Let us consider next the dimensions of this additional load.

Modern psychiatry has discovered that the neurotic process is literally universal in every culture which it has had an opportunity to study. It has demonstrated that in the present state of knowledge we do not know how to bring up children free from that gradual, intermittent, and recurrent deformation of the symbolic and affective processes, of which the neuroses of infancy and childhood are manifestations, and whose cumulative end-results are the neuroses and ultimately many of the psychoses of adult life. Certainly any such universal source of frustration, despair, educational failure, irrational behavior, illness and human misery, of psychosomatic complications of organic illness, of delinquency, criminality, psychopathy and perversion deserves to be known as Public Enemy No. 1.

In 1950 the total school enrollment was over 30 million; and in 1960 it is expected to be close to 40 million, a 33⅓% increase. One research group (Columbia Univ. unpublished), has claimed recently that a sample of the public school population of this country shows that at any one time at least 10% of the school children (i.e. 3,000,000) are in immediate need of psychotherapy, while other tenths have needed it in the past and still others will need it in the future. Thus the size of the potential psychotherapeutic challenge becomes staggering, even if we limit ourselves to the preventive application of psychotherapy

in early life, in an effort to forestall the cumulative neurotic illnesses of later years.

Statistical studies by Lionel D. Edie & Company (4), confirm the data given above, indicating that by 1960, as against 1940, there will have been a *35%* increase in the school age population of 6 to 17 years. These same studies add still another important consideration. During the same two decades there will have been a *63%* increase in the age group of 60 years and over; while the total population will have increased by only *26%*, and the age group between 18 and 60 (i.e. the adult wage-earning years) by only *17%*. Thus the triumphs of modern medicine are confronting us with an enormous increase not only in the economic burdens of the wage-earning years, but also in the psychotherapeutic challenges of childhood and adolescence at one end, and of the age group over 60 at the other.

The psychotherapeutic needs of this older age group are certainly not unfamiliar; but its increasing dimensions are new. The work of Goldfarb and Sheps, and others (6), (7), (8) and of Kubie and Landau (20) demonstrate its accessibility and its responsiveness to psychotherapy. It has always been known that some human beings mellow with age. Recent experiences show that a not inconsiderable number of human beings become increasingly accessible to psychotherapy and even to deep psychoanalysis as they grow older. Some who were unanalyzable at 20 or 30 become analyzable at 60. Therefore as we face the many problems which are created by this progressive extension in the life-expectancy of the older age groups, we must not shut our eyes to the fact that this rapidly expanding segment of the population will need both deep and superficial, individual and group psychotherapies. Thus at both ends of the age spectrum, we are confronted by a human need of vast and increasing dimensions.

Parenthetically it is worth noting that the increasing numbers of people in the older decades is creating not merely a huge new therapeutic challenge, but also an astonishingly rich source of research material. Many of our hypothetical reconstructions of infancy and early childhood are being re-lived under our eyes, as the aged return to the battle-grounds of their nursery years. The research value of this material is incalculable. Its systematic investigation is a scientific "must." If it is to be studied at all,

this must be done without undue delay, since once the processes of arteriosclerosis and the biochemistry of aging are solved, these data will again be lost to view.

B. What of Prevention as a Solution?

Discouraged by the immensity of the problem, some educators try to dismiss it from their minds and at the same time to ease their consciences by saying that the answer lies in prevention rather than therapy. In the future this will be true, of course. Furthermore we can agree that if civilization survives at all, prevention will ultimately depend upon new methods of education and profound cultural changes. Unfortunately for these apologists for inaction, however, not only do such cultural changes come slowly, but their very advent is initiated and made possible only as a by-product of cumulative individual therapeutic experiences. What is more, prevention today has only one effective and immediately applicable tool, namely early therapy: therapy for the many for whom it is not now available; therapy for young parents, therapy for young children themselves, and therapy for the older group. Only through early therapy can mankind learn the lessons which it may in the end learn to use prophylactically, i.e., so as to modify the cultural, familial and educational influences which shape childhood. Finally, the wise application of such new knowledge will always demand the enlightened participation of parents; and this also requires early therapy. Obviously then there is no other path for human progress. Therefore no optimistic visions of ultimate prevention lessen the urgency of the immediate need for *early diagnosis and early therapy*.

C. The Potential Sources of Future Specialists

But who are those who can be trained to grapple with this problem? Naturally the first question is whether they can be recruited from the ranks of medicine.

The American Medical Association has recently been involved in a controversy about whether or not we will run into a shortage of physicians in the near future, and whether the slowly expanding resources of our medical schools will prove to be sufficient to meet the growing needs of our population for more physicians

and surgeons. In the study of this problem statistics have been gathered by many different groups; by bio-statisticians from medical schools, by statisticians employed by the American Medical Association, by special committees under the armed services (1) and under the Federal Security Administration, etc. (22, 23). These statistics have been used to prove both that we will soon have too few doctors and also too many. The data itself is inconclusive. Fortunately for the purposes of this particular thesis there is no need either to summarize these loaded arguments, or to attempt to evaluate them; since the differences between the various estimates are not large, and also because *even the most optimistic of them merely demonstrates that there can never be enough doctors to man our psychotherapeutic guns.* The most that anyone claims is that whereas in 1940 we had 133 physicians per 100,000 population, in 1960 we may hope to have 143 physicians per 100,000. It is comforting to anticipate also that better means of transportation, better organization of medical care, and a more economical use of the physician's time will enable these 143 physicians not only to do a better job qualitatively, but also to make more economical use of their time than was possible for their predecessors. Let us therefore assume arbitrarily that such economies can augment this gain of 10 physicians per 100,000 population by freeing perhaps 15 others from medical and surgical responsibilities to devote themselves to psychodiagnosis and psychotherapy. Even if this optimistic assumption should prove to be true, it would still be inadequate to meet the psychodiagnostic and psychotherapeutic needs of this booming population of youngsters and oldsters in the American scene of 1960.

Dr. Ginzberg presents a conservative and critical analysis of the statistics gathered by the President's commission (5). He makes it evident that one must use all such statistics with great caution, regarding even seemingly precise data as tentative approximations. Thus depending on the sources and how the data was assembled the commission's estimates of physician shortages in 1960 varied between 22,000 and 45,000. Evidently, as Ginzberg points out, "as small an improvement as 10% in the utilization of the existing supply of physicians" would cancel out most of the lesser of these two estimates of future deficits . . . Evidently, however, for the purposes of the thesis advanced in

this paper, what is most significant is the fact that not even this conservative and optimistic estimate anticipates that future economies in the utilization of medical personnel will yield a *surplus* of physicians, who could then be retrained to fill our expanding psychiatric needs.

Let us nevertheless test the validity of our argument by pushing optimism still further. The Bureau of Medical Economic Research of the American Medical Association assumes that by 1960 the "productivity" per physician (whatever that may mean) may have increased by as much as one-third. Clearly this would not release one-third of our total medical force from medical and surgical responsibilities, unless every current need for essential medical and surgical care was already being served adequately by the existing medical force, an assumption which no one makes. In 1950 we had 200,000 physicians. It is estimated that 250,000 will be practicing in 1960. The annual product of all of our medical schools is approximately 6,000 graduates. Therefore, between 1950 and 1960 we will have graduated approximately 60,000 medical students. Death, retirement and illness will eliminate about 10,000 physicians in this decade. Therefore in these 10 years our medical schools will just be able to maintain a supply of physicians and surgeons to meet the expanding population. How at the same time can they add materially to our psychiatric forces?

At present there are approximately 5,000 practicing non-administrative psychiatrists in the country. If anticipated economies in medical procedure will release 10% of the new graduates to turn to psychiatry, we would only have doubled our present psychiatric forces by 1960. In this decade we could not turn the entire product of our medical schools into psychiatrists. Nor can we retrain any considerable number of older internists or other physicians to perform psychiatric functions. *The conclusion which is forced upon us is that the medical profession as constituted at present or in foreseeable decades cannot supply the community's present or future needs for psychodiagnosis and psychotherapy.*

As a matter of fact, quite apart from all such statistics on the medical profession, there are other reasons which force us to the same conclusion.

In the first place, the medical curriculum is steadily becoming more heavily burdened with new, complex technical disciplines and sub-disciplines which the medical student must master. Realistic medical educators are already wondering whether this can go on indefinitely, or whether the medical curriculum may not have to be condensed and subdivided, much as the engineering curriculum has been subdivided, with a trunk which is common to all, but with early curricular branching in various special directions. Everyone who contemplates the possibility that this may become necessary is troubled by its obvious drawbacks. Yet there is no escaping the fact that the medical student is already so overburdened that serious if reluctant thought is given to ways in which the curriculum might safely be divided into special fields at an earlier period. Under these circumstances any plan would be unrealistic and impractical if it involved overburdening still further the hapless medical student by adding intensive training in psychodiagnosis and psychotherapy, (a training which will make additional demands upon his time, thought, energy and attention) or if it involved extracting time from other fields of study.

What then of the practicing physician and surgeon, whether internist or specialist? Can he be trained or re-trained to do his own psychodiagnosis and his own psychotherapy along with his regular medical work? Experiments in this direction have already been made under the auspices both of the Commonwealth Fund (3), and of the Army (2). Also in recent years, many of those who are interested in the field of psychosomatic medicine have held out hopes that the supply of psychodiagnosis and psychotherapy can be increasing by making every physician his own psychiatrist. This is wholly unrealistic, wishful thinking. Let me enlarge on what I have written about this in another place (13).

Under the banner of psychosomatic medicine, it is popular to say that a great deal of psychotherapy can be taken care of by these men (i.e., by internists and surgeons, etc.). I too, wish that this were possible, because then our whole problem would be simpler; but for several reasons I am afraid that it cannot be solved in this way. In the first place, it is difficult for one man to play both roles with any patient. In the second place, the average internist and surgeon is already under heavy pressure. Medical

and surgical patients require a great deal of unpredictable time. Psychotherapy requires a steady routine, which cannot often be altered to meet the transient and episodic emergencies of other patients. Furthermore psychodiagnosis and psychotherapy require a slow and patient pace. They cannot be pushed rapidly. Words which are helpful and illuminating when they are said slowly and spaced out over days, become destructive when hurried. Histories which are gathered slowly can be therapeutic; whereas the same life-story, if gathered in a rush with drumfire qestioning, can be a disastrous experience for a patient. Yet the pressure under which the internist and surgeon works makes it impossible for him to give that amount of time to each emotionally needful patient. He simply does not have it. With the best will in the world it is impossible. If he hurries, he does harm. Yet from the first day of his internship every young house officer is taught to hurry. Indeed he is made to feel proud of his speed in history-taking, instead of ashamed of it. This bit of mis-education is not easily dislodged, and often ruins for the rest of his professional career a young physician's potential psychotherapeutic leverage.

The other psychotherapeutic error that the general man, or the internist, or the surgeon is most likely to make is to gloss over things. If he is astute he can frequently aid a patient temporarily to rid himself of certain symptoms; but merely to cure symptoms often obscures a disease process (10), (14), postponing the day when it will break out in more serious form. Instead of using the initial "neurotic" symptoms as a warning signal, i.e., as an opportunity to attack the roots of the neurosis while it is still young and fresh, superficial therapy frequently enables the nascent illness to go back into hiding; so that by the time that such a patient finally reaches the psychiatrist, after months or years of hasty and inadequate psychotherapy from a busy internist, he usually brings a life that has been strained hopelessly out of shape in a battle with a neurotic process that has been allowed to run its own course for years. Obviously this creates a much tougher therapeutic challenge.

These facts bring us to the unwelcome yet inescapable conclusion that it is impossible to place this burden on the medical profession alone, neither on the medical curriculum for added

training, nor on the individual medical practitioner in the midst of his over-burdening daily tasks.

How then, can the number of fully trained psychiatrists be increased to meet the need? We know that psychodiagnostic and psychotherapeutic services are needed by newlyweds, by young parents and their young children, by all social agencies, by courts and penal institutions, by all educational institutions (i.e., day-care centers for infants, nursery schools, kindergartens and on up through the entire educational system, to and through our graduate schools); and also by every institution, from birth to the grave, which offers health services, i.e., in obstetrical and pediatric services, in all general hospitals, as well as in all psychiatric out-patient clinics and in psychiatric hospitals, and in homes for the aged.

There are many estimates as to the number of individuals needed to service these needs. According to the American Psychiatric Association to service our mental hospitals alone the minimal personnel requirements is 10,000 to 20,000. For this task we now have between 4,000 and 5,000. To train psychiatrists adequately for this type of routine hospital psychiatry of the psychoses takes at least 5 *years after graduation from medical school*, and usually longer.

Furthermore, as psychiatry leaves the mental hospital to tackle the problems of community psychiatry (i.e., of preventive therapy of the neuroses, child psychiatry, psychosomatic medicine, and education) we will need at least another 20,000 to 25,000. The processes of training for this take even longer. It has been conservatively estimated that for this type of work no medically trained psychiatrist finishes the essentials of his training in less than 8 *years after graduating from medical school;* and that for most it takes nearer 10. If full psychoanalytic training is included we must measure the duration of training in terms of 12 to 14 post-college years; after which further years of clinical experience are required before the psychoanalytic psychiatrist reaches full maturity in his field. By this time he is usually somewhere between the ages of 35 and 45.

The practical effects of this are to reduce the number available to the community, to reduce the numbers that each training facility can turn out, and to reduce the span of years in which the fully trained psychiatrist can practice the profession which

he has spent so many years in mastering. This also jeopardizes and limits his own economic security; because by the time he finally emerges from training, too few years are left to him in which to achieve economic independence for his old age, or to provide for his children a level of education comparable to that which he has enjoyed. These are the hard facts which make psychodiagnosis and psychotherapy by the fully trained medical psychiatrist both scarce and expensive. Clearly then the present system creates a scientifically, medically, socially and economically intolerable state of affairs.

Anyone who will study with an open mind the statistics on population distribution and longevity, the statistics on the production of medically trained personnel in general and the special statistics concerning the production of medically trained psychiatrists, and who will also contemplate the ubiquity of the neurotic process in adolescence and old age, and the importance of early psychodiagnosis and early psychotherapy for prevention, must be convinced that some new source of students must be found, and some new way of enabling men to achieve maturity as human beings, maturity as research scientists, and maturity in the specific techniques of psychodiagnosis and psychotherapy. These are the many considerations which have forced me to the conclusion that society needs a new profession, based on a new curriculum, which will enable more men to become competent practitioners of psychodiagnosis and psychotherapy at an earlier age (11, 12, 13).

D. Teachers

By whom will these individuals be trained? It is clear that as of this moment we do not have enough people to train even our current meagre crop of psychiatrists. Therefore, it is impossible for the same teachers to train additional students in large numbers, especially if these students come with even less preparation than does the average young physician who presents himself for training in psychiatry. The physical plant and the teaching beds in our hospitals, clinics and supervised agencies could take care of them. But there are not enough mature teachers to conduct the group and individual supervision which this training requires. Obviously, then, the first step must be to train teachers.

If we were to start from scratch, this would take even longer than the training of practitioners. Fortunately however, the program could be launched by giving additional training to experienced practitioners, to start building the nucleus of a teaching corps. Inevitably a certain percentage will drop out, either because they prove not to have the facility to work in this field at all, or because they will be seduced into practice by the financial remunerations, or because they prove to be sound practitioners but inept teachers. Therefore, even the number of competent teachers could not grow quite as rapidly as we would wish. Yet there are many individuals who have had long experience in psychiatric social work, in clinical psychology and in education, and who over the past years have been able to add to this a considerable amount of experience with clinical psychiatry and in dynamic and psychoanalytic psychotherapy. Most of them are already in practice; but some could be rescued from practice for an additional period of intensive training. Surely in five to ten years these could become a hard core of able and experienced teachers. This would not produce an immediate flood of well-trained practitioners; but it would make a start.

In spite of such possibilities as this, there are some defeatists who feel that there can be no solution of any kind to this problem. They buttress their pessimism by emphasizing the bottleneck which results from the paucity of experienced teachers of psychodiagnosis and psychotherapy. Certainly this is one of the reasons why the general effort to expand psychiatric services progresses slowly; but there are others as well. For instance the rapid expansion of departments of psychiatry in medical schools has drained into administrative work and into elementary medical school teaching most of the promising young brains that should have been allowed time to mature in psychodiagnosis, in psychotherapy, and in research (17). Moreover as was first pointed out during the post-war years, it is not possible to train more psychotherapists without for a limited time sacrificing a certain amount of psychotherapy (11), since this instruction must be given by experienced psychotherapists in time which they withhold from practice. With our present manpower we cannot provide all of the treatment which the community needs,

while at the same time providing the training which a rapid expansion of these services would require.

This same principle holds true with respect to the program which I am outlining. Therefore it cannot start by training the annual current crop of new college graduates. It must start by providing advanced training to older men who have already had broad experience in clinical psychology, and in many instances in psychodiagnosis, psychotherapy, and psychological research. If these experienced men could be endowed for a period of even five years of intensive additional training and supervision, they would make a nucleus of mature and competent teachers to start us off. This would entail a temporary sacrifice of therapeutic time; but the nucleus of future teachers would multiply by many times the treatment personnel available for the future. Moreover it should be borne in mind that much of this teaching would take the form of on-the-job instruction, during which the students would conduct supervised treatments. This would be particularly true for this initial group of students. Therefore, the initial sacrifice of available therapeutic time would not be as large as might otherwise be expected.

These advanced students need not be recruited exclusively from the ranks of clinical psychology. They could come also from among the lay psychoanalysts, and from psychiatric social workers with a broad clinical experience in personnel work, guidance work, group psychotherapy, and counselling. A few men from other backgrounds as well could be drawn into the program, so that the initial training staff would represent not only psychiatry, psychoanalysis, clinical psychology and psychiatric social work, but also kindergartners, educators, cultural anthropologists, ethologists, philologists, etc.

For such an heterogeneous but experienced group the details of the curriculum cannot be rigidly laid out ahead of time; since many of them would bring to it a great deal of prior experience, both clinical and theoretical. Furthermore because of their psychological maturity, the biological and clinical phases of their training could be given in more concentrated doses than will be possible for later groups of students, most of whom will come to training as recent college graduates.

Five or ten years of concentrated training of teachers, administered in a few key universities and medical schools, would produce many new teachers, to make possible the future development of this new profession.

E. The Logistic Results

If the facts which are presented in the preceding section are accurate, this plan will supplement existing programs for the training of medical psychiatrists with a steadily growing stream of Doctors of Medical Psychology. For reasons which will be presented in Section VII, I believe that the training of medical psychiatrists will continue not merely unabated, but actually at an increasing rate, as the new profession yields more experienced teachers and investigators as well as practitioners. Nevertheless, as in the past this will require ten to fourteen years after graduation from college. The training of the new Doctors of Medical Psychology would by contrast take in the neighborhood of seven or eight post-graduate years. Thus as soon as the available supply of experienced teachers and research personnel will have been materially increased, existing facilities will be able almost to double their present annual output; and over the course of several decades the annual output of trained practitioners and scientists would continue to increase steadily, finally reaching a plateau only when the future needs of the community are approximately at a saturation point. Not only would this have a salutary effect upon the quality and availability of men trained for participation in every application of dynamic psychiatry to community needs, but in addition their impact on our whole culture would be invigorating. Many more individuals would have an opportunity to become aware of the role of the universal neurotic potential (14), as this influences every aspect of human life. The deeper knowledge of human nature would no longer be confined to psychiatry and medicine, nor to such special aspects of medicine as pediatrics, nursing and social work. Instead, it would be available for all education, where as we have said it is impossible to separate the processes of learning from either the neurotic process or the process of therapy. To appreciate the immediate importance of this in childhood one needs only consider the role that the neurotic process plays in the daily struggles of teachers and of parents with the obsessional dawdler, with the

phobic child, with the compulsive child and student, with neurotically generated boredom, and with all anxiety-driven distortions of student attitudes toward education.

In other words, this spread of knowledge would influence every cultural attitude. To mention only a few, one might think of the false dichotomy between work and play as an expression of neurotic components in human nature; of vocational choices and aptitudes; of the role of neurotic forces in art, literature, dress, and money matters, etc. Such a cultural infusion would influence the ministry and pastoral counselling. It would enter into the field of jurisprudence. Thus, wherever we are challenged by the subtle but universal manifestations of psychopathology, this infusion of knowledge would be fruitful. Under these circumstances it is a presumption for any single discipline to stake an exclusive claim to the only type of experience by which it is possible to acquire an understanding of psychodynamics, of psychodiagnosis, of the processes of falling ill and of falling well. This knowledge must become part of our common human heritage, infusing every aspect of education from infancy to its highest levels. It must become part of the process by which Everyman matures. Consequently, representatives from all cultural disciplines must be exposed to its influence. If the ultimate goal of education is to give man a chance to change, then everything that we have learned and are attempting to discover about the dynamics of psychological health and illness, must become part of universal education. This goal, too, will be served by the program which is to be outlined here.

IV. A Solution: A Doctorate in Medical Psychology

These many considerations bring me to the conclusion that we need a new profession: to wit, a Doctorate in Medical Psychology. A curriculum for such a profession and also a legal basis for its practice have been outlined in general terms on other occasions (12), (13), (21); but they will be described in greater detail here.

A. The Students: Now and in the Future

Ultimately, most of those who study for a Doctorate in Medical Psychology will make their first tentative moves in this direc-

tion while they still are undergraduates in college. Just as anyone who hopes to study medicine must satisfy certain premedical requirements in his undergraduate years before his application for admission to medical school can be considered, so the student who hopes to become a Doctor of Medical Psychology will have to bring to his professional education at least an introductory grasp of certain basic chemical, physical, biological, mathematical, sociological, anthropological and psychological sciences. These studies explore the range of his interests and aptitudes; screen out incompetents, and prepare him for later advanced studies in the same fields. Naturally individual applicants vary widely in the emphases of their undergraduate years, as is true among applicants for admission to medical school. Some will have concentrated in the humanities, some in the psychological or sociological sciences, some in biology and the more exact sciences.

I have already pointed out that during the first five or ten years in the development of this new profession, it will be essential to concentrate on the training of teachers. During this preparatory phase only experienced clinical psychologists, with an M.A. or a Ph.D. or its equivalent will be accepted. Later on, however, most of the students who are to go through this curriculum will come only with a bachelor's degree, just as medical education is based on a B.A. or a B.S. and not on previous graduate degrees. Undoubtedly many techniques will be tested for selecting candidates, such as those developed at the Menninger Clinic and elsewhere; but it would be premature to make any specific suggestions along these lines at this time.

The college student whose interests turn first to academic psychology and subsequently to the alleviation of psychological suffering, rarely recognizes what it is that draws him in these directions. Usually he knows nothing about psychiatry, and does not understand the differences between a psychiatrist and a psychologist. It has rarely occurred to him to become a doctor. Furthermore, if any considerable number of such students were to apply for admission to medical school the majority would be turned down, not because of any lack of aptitude, but because the available openings are limited and are not increasing in number. Therefore, in a sense it is fortunate that in their undergraduate

years most of them think only in terms of academic psychology. Later on, in increasing numbers they gradually turn toward clinical psychology, where their therapeutic zeal first is kindled.

In another communication I have described this transition in the following terms (13):

"What is the clinical psychologist, and what role does he play? In the first place, he can perform any of several functions depending partly on his previous training and background, partly on his individual gifts, and partly on the special situation in which he happens to be working at any particular time. He may be (a) an expert in the routine use of certain complex diagnostic psychological instruments. Here his function is analogous to that of the electroencephalographer, the cardiologist, the radiologist, the clinical laboratory man, etc. Or (b) he may be a member of a research team, using similar instruments for special studies of sociological, psychiatric, or psychosomatic problems (*i.e.*, cultural, economic, medical, surgical and psychological interrelationships). Here he may be called on to examine random, cross-section samples of the populations of hospital wards, or of various groups in the community, or of special groups with special talents, or of special illnesses, etc. Finally (c) he may be a member of a therapeutic team.

"In practice, these three roles always overlap; and the investigator of human misery can rarely, if ever, avoid assuming some therapeutic responsibilities. Forces arising within the psychologist himself and the external pressures which arise out of human needs together compel him to intervene in the problems of those he tests and examines. Indeed, the greater his diagnostic acumen, the greater is this trend. The test instruments of the skillful clinical psychologist bring into clear focus many obscure elements in the constellation of a neurosis. Latent compulsions and latent sensitizations which often escape superficial examination come into view. Quantitative impressions are gained of the relative strengths of various unconscious forces, thus helping to indicate the alignment of different facets of the personality. Consequently, if the clinical psychologist is himself a normal human being with normal human sympathies, whenever he summarizes his impressions of a patient's neurosis, he must inevitably weigh the possibilities and means of therapy. Frequently he sees an op-

portunity for early preventive treatment which might avoid years of illness. There would be something defective in the emotional make-up of the clinical psychologist whose mind turned away from this human need and was contented with diagnosis alone. In psychiatry and clinical psychology, perhaps even more than in internal medicine, there can be no such thing as a diagnostician devoid of therapeutic impulse. . . .

"This therapeutic impulse is found in all of those who practice the psychological disciplines, whether as psychiatrist, clinical psychologist, or psychiatric social worker. We come into this field weighted with the emotional residues of our own earlier internal stresses. Consequently we identify with patients and try to cure ourselves in curing them. In this there can be much danger; yet without some measure of such identification there is reason to doubt whether anyone could practice psychotherapy effectively. At all events, for better or worse, this intra-psychic pressure is present and operates in every good psychiatrist.

"Furthermore, the clinical psychologist is up against the practical fact that having recognized and described a patient's neurosis, and having in some measure identified with him, more often than not he has no place to send the patient for treatment, whether privately or on a clinic basis. Under such circumstances, it is inevitable that the best workers in the field of clinical psychology will find it impossible not to attempt to help the patients they examine." . . .

It is important not to misunderstand this point. It would be foolhardy to argue that everyone should be trained to heal who wants to heal. This impulse like anything else can itself be neurotic. Among those who feel the inner drive to heal are some who have an inner gift for relieving psychological suffering. Yet no matter how great their gift, without technical training they remain not merely ineffectual but potentially destructive, just as a man with skillful hands would be destructive if without training he attempted to perform surgical operations. Skillful hands need meticulous training before they can be trusted with a scalpel; and the same principle holds true even for the highest, native psychological aptitudes. The problem is how to select, how to train, and how above all to turn the dedicated therapist into a scientist.

Before considering training in detail, it is important to point out that no individual can expect to enter the field of clinical psychodiagnosis and psychotherapy, and at the same time remain active in any other field. Dr. Abraham Franzblau, Professor of Pastoral Psychiatry and Dean at the Hebrew Union College of the Jewish Institute of Religion of New York and Cincinnati, spelled out the reasons for this in an able letter to the New York Herald Tribune of February 26, 1954. He limited his discussion to the role of the minister in psychotherapy; but his argument applies to other disciplines as well. He wrote: "The minister who practices psychotherapy in his congregation cannot maintain his customary role in the pulpit, school, pastoral work, or the organizational life of the community. He finds himself cast in inconsistent and conflicting roles. . . . He will be the butt of conscious and unconscious therapy-generated hostilities . . . without having the advantage that the psychiatrist enjoys, of secluding himself. He must also by the very nature of his calling, be available to all. He cannot cure sick psyches of congregants while he is himself professionally a split personality." This point applies to other professions, and indeed to other medical specialists as well. We cannot mix these roles; because scientific psychotherapy requires a relationship to the patient which must in the main remain uncontaminated by such other functions as counselling, advising, educating, testing, joint research, physical care, etc. Therefore, any plan to supplement the supply of psychotherapists by training representatives from other behavioral sciences must inevitably have the effect of draining individuals from these other disciplines. The practicing therapist leaves his previous field, undergoes additional training which he had previously lacked, and acquires a new equipment which he will now use in his new role. Thereafter he cannot keep one foot on base, trying to be both the old and the new. This is not generally understood; yet it is one of the basic reasons why it is important to clarify the position of the psychotherapist in the eyes of the community by granting him a special degree, which signalizes his membership in a new profession.

B. The Curriculum

(1) *The Subjective Component:*

I would like to see each student inducted into his training through some form of psychotherapeutic experience. This could

take many forms. I would favor at first the use of sessions of *group therapy*, to be held at least once a week under the leadership of an experienced therapist. This would be comparable to the experiments which Carl Whitaker and his group at Emory Medical College in Atlanta have conducted among medical students. Sessions of group therapy for the students might continue throughout the first three years, paralleling their growth in technical maturity; until finally, and as a part of their own further training, the students were ready to undergo individual preparatory psychoanalyses, and also to conduct psychotherapeutic groups for patients under the supervision of more experienced therapists. In this way the subjective therapeutic experiences would merge into objective training in group therapeutic techniques and into deeper individual psychoanalytic training.

(2) *Objective Components:*

The curriculum for the Doctorate in Medical Psychology would consist of two parallel courses of training under joint auspices. The more strictly psychological aspects would be conducted in the graduate departments of a university; while the courses in the basic medical sciences and the clinical indoctrination would be conducted in medical schools and teaching hospitals. These parallel courses will be outlined below.

(A) *Basic Medical Science:*

The first year would be devoted largely to a course in basic medical sciences comparable to that which is given in the Harvard Medical School under Dean George P. Berry. This curriculum is designed specifically for those who want to master the basic medical sciences for purposes of teaching and fundamental biomedical research, but who do not plan to practice medicine. It would consist of:

(a) An abbreviated course in human anatomy, which would make use of all time-economizing visual aids, transparent models, and diagrammatic methods, with relatively little dissection.

(b) A course in the physiology and biochemistry of cellular structures. The structural basis of microbiology would be taught largely through the projection of slides and diagrams, with little time on microscopic techniques of fixation, impregnation, cutting, staining, mounting, etc.

(c) Emphasis would be on *physiological chemistry, clinical physiology, the essential principles of pharmacology, clinical neurophysiology and endocrinology.*

From this basic curriculum, immunology, allergy, bacteriology and clinical pathology would be omitted, except for their general principles.

(B) *Basic Clinical Psychology:*

In the University, courses in psychological testing, in social psychology, and in cultural anthropology would parallel the courses in the basic medical sciences. Whether these would be introductory or advanced courses would vary with the previous training and experience of the individual student. Whether they would be given in the first or second years would have to be dictated by experience.

(C) *Basic Clinical Experience:*

(1) *Experience with Organic Diseases.*

The second year would take the student directly to the wards and out-patient clinics of a general hospital for one-half of every day, to work there with patients suffering from organic disease. He would serve first as a relatively unskilled orderly, ward attendant or nurse's aid. Subsequently he would administer to random samples of the ward or clinic population the psychological tests which he has been learning. Then he would learn to take psycho-social histories under the joint supervision of house-officers, psychiatric social workers and psychiatrists. Thus he would gradually come to serve as a psychological interne or extern, to aid both the medical students and the house staff, by whose side he would be working every day, and to whose case presentations he would contribute on rounds by the bedside, in mixed staff conferences, and in formal clinic presentations. This opportunity to grow up with medical students, to share in their learning experiences, to live and to work in close daily association with physicians and surgeons, to become their junior partners, to imbibe the atmosphere of hospital and medical school life, and to face daily the facts of illness and death would constitute a vital ingredient in the cultural training and in the emotional maturation of these students. It would generate in the future Doctor of Medical Psycology that dedication to therapy and at the same time that skep-

ticism about naive therapeutic claims which together mark the mature physician. It would also give him a sense of familiarity with organic disease and pathophysiology which would be essential to him in all of his later activities. It would make him at the same time seriously cautious yet unafraid of the somatic complaints of his future patients.

At some point the student would make home visits under the guidance of medical social workers, so as to deepen and broaden his awareness of the familial and social impact of organic illness. Through these successive experiences, organic illness would become concrete to him, which would provide a solid and realistic basis for later studies of the subtle symbolic significance of physical disease.

(2) *Introduction to Psychodynamics:*

Paralleling this he would begin his didactic training in psychodynamics; *i.e.*, in the dynamics of human development in general, in the psychological factors in the development of health and illness, and in fundamental principles of clinical psychodiagonosis and psychotherapy.

(D) *The Third Year:*

After these two years of exposure first to general human biology and then to the realities of organic illness, the further steps in clinical training would be transplanted to a psychiatric setting. Over a period of years this would move by successive steps through the wards of a psychiatric hospital, to its out-patient department, to treatment centers for disturbed children, thence to schools, courts, and social agencies.

On the psychiatric wards he would go through a series of steps paralleling his experiences on organic wards: serving first as an orderly or attendant, then as a nurse's aid, then administering psychological tests, then taking psychiatric histories under supervision, and finally visiting the homes of patients under the guidance of the psychiatric social workers. This would give him an opportunity to familiarize himself with the lives and problems of psychiatric patients, through his daily contacts with the patients themselves, through the test instruments, through the amnamneses, and through direct contact with their homes and their settings.

Concurrently he would parallel these clinical experiences with advanced psychological training, including research methodology.

(E) In his fourth year he would expand this basic psychiatric experience by turning from the psychoses to the neuroses in various settings. At the same time he would receive further training both in testing and in history-taking, plus his first supervised experience in leading psychotherapeutic groups. . . .

In my judgment, he should at this point also start his personal psychoanalysis. This step in training will continue to be essential until and unless it should be demonstrated that his previous three years of group psychotherapy had served the same ends more economically. About this it is legitimate to base some guesses on theoretical expectations; but it would not be legitimate to allow ourselves to make definitive pre-judgments (2) (15). The personal analysis would be supplemented by continuous case seminars, by lectures on psychoanalytic history, theory and technique, and finally by an opportunity to conduct analyses of selected patients under supervision.

(F) *The Subsequent three or four years* would gradually take in the clinical problems of all ages and in all the varied social settings which have been listed above. As they pursued their full psychoanalytic curriculum the students would become familiar with the neurotic disturbances of childhood, adolescence, adult life and old age; in schools, courts and social agencies, by testing, history taking, diagnosing, and treating.

As we review this curriculum in perspective, we will note that after the first year, at least one-half of each day is devoted to direct work with patients of all kinds, the other half to more theoretical aspects of psychological, psychodynamic and biological sciences. Gradually over the seven or eight years an increasing emphasis would be laid on the methodology of critical experimentation and on the statistical evaluation of theories and results, whether these be biological, physiological, neurophysiological, psychological, sociological or psychoanalytic.

In the above outline, I have been more concrete in my suggestions concerning the biological and clinical components of the curriculum than about its academic components. For the detailed

planning of this aspect of such a curriculum our psychological confreres are more competent than I. The integration of physiological chemistry and clinical physiology with clinical psychology, clinical psychiatry and dynamic psychiatry would be emphasized consistently in relation both to the dynamics of psychological illness and to research on etiology and on therapy.

It does not seem unrealistic to anticipate that the graduate of such a curriculum as this would be fully capable of making independent psychodiagnoses and of conducting independent psychotherapy. The patient would be protected by the legal requirement that there must be periodic medical check-ups, to eliminate any possibility that coexistent organic disease would be overlooked; and this not only during the preliminary study of a case, but also recurrently throughout the course of treatment. Nor would the graduate of such a curriculum require direct supervision from a medically trained psychiatrist. Better and more adequate safeguards would have been achieved both through the quality of his training and through the legal requirements of his license. Therefore I will now spell out these safeguards, borrowing from the deliberations of certain special committees of the Group for the Advancement of Psychiatry, and of the American Psychiatric Association, which on several occasions have made excellent tentative formulations of such safeguards.

V. Standards and Safeguards

Any new approach to the solution of our over-all problem should maintain existing standards, and should also hold the door open to further gradual improvements, at the same time safeguarding the welfare and interest of patients against various types of error. Furthermore, it must serve the general needs of society as a whole.

(1) In the long run, the raising of clinical standards presupposes high standards of basic research, which in turn requires a steady supply of men who are mature not only in research techniques, but also in diagnosis and therapy (17). Consequently this plan must not create a cleavage between research and therapy, nor between the organic and psychological approaches to psychopathology. Instead, it must ensure their closer integration, first through a curriculum in which the biological, psychological

and clinical are well balanced, and secondly by creating a legal framework which will make mandatory the close working integration of clinical physiology, clinical medicine and dynamic psychology, which is one of the unique achievements of American medicine.

(2) The individual patient must be safeguarded against the possibility that organic illness and/or psychosis may be overlooked.

(3) The public at large must be protected from incompetence, and from irresponsible, naive, exaggerated, and generally unscientific therapeutic claims.

(4) A steady supply of fully trained teachers and practitioners of psychodiagnosis and psychotherapy must be assured by making these careers lawful, dignified, and economically secure.

(5) From the program as a whole a steady diffusion of psychiatric knowledge into the blood stream of our educational system and of our whole culture must result.

No plan can be a valid solution of our problem, unless it serves each of these goals. Therefore, my task is to demonstrate how the solution which I am suggesting will fulfill these exacting requirements.

In psychodiagnosis and psychotherapy adequate safeguards must protect against the risks of overlooking concomitant organic disease, which may be either contributory to the neurosis or concomitant with it, and which may antedate the patient's initial consultation or may arise during the course of therapy. Consequently the safeguarding procedures must be initiated at the time when the patient first comes for diagnosis, and must be maintained thereafter by periodic, systematic checks not only through the period of evaluation, but also throughout the program of treatment. Therefore, any doctor of medical psychology who practices psychodiagnosis and psychotherapy must maintain a close working relationship with competent physicians who will subject his patients to meticulous organic scrutiny during the initial period of investigation, and periodically throughout the course of treatment.

Similar safeguards must be maintained against the possibility of overlooking coexistent psychotic disturbances, which can be masked by maladjustments which appear to be more super-

ficial. The sudden eruption of psychoses and of suicidal, homicidal, or other dangerous impulses cannot always be anticipated or prevented, even by the most expert medically-trained psychiatrist, exercising the greatest therapeutic caution. Moreover, the needs of therapy itself sometimes demand that "surgical" risks be taken deliberately, as calculated risks of psychotherapy. Any doctor of medical psychology who has been trained in this curriculum will also have had enough experience in dealing with the psychoses to be sensitive to these dangers. Therefore his judgment on the degree of risk should be as accurate as that of any fully trained psychiatrist, since in this curriculum the doctor of medical psychology and the medical psychiatrist would have gone through identical psychiatric training and identical clinical experiences. The fact that a doctor of medical psychology would not be able to diagnose cirrhosis of the liver would hardly lessen his ability to diagnose or recognize an impending suicidal risk in a depressed patient, nor to recognize a psychosis masking itself as a neurosis.

Since psychotic explosions occasionally occur, in spite of the highest skill and training, the doctor of medical psychology will have to establish routine procedures for taking care of emergency hospitalizations as needed. Without direct access to hospital facilities, the mere ability to recognize the coexistent danger of a psychosis will not protect the patient. For purposes of emergency hospitalization, every doctor of medical psychology will have to have a close working liaison with a psychiatrist or a mental hospital. Otherwise it will be necessary to grant him the legal right to sign commitment papers, so that he could take care of emergencies himself.

As a matter of fact, it is worthwhile at this point to remind ourselves that the same problem frequently arises in private psychiatric practice. Any psychiatrist who for many years has limited himself largely to the psychotherapeutic and/or psychoanalytic treatment of the neuroses, gradually loses much of his contact with psychiatric hospitals and with the procedures of hospitalization. Then when he suddenly is faced by a need for an emergency hospitalization, either during the course of the treatment of one of his borderline patients or during a consultation, he is apt to flounder and lose valuable time, and often is

unable to deal with the situation any more effectively than a non-medical therapist. Among those who practice office psychiatry only those who also maintain a close liaison with mental hospitals can take care of such emergencies expeditiously. Therefore any plan by which emergency hospitalizations can be taken care of promptly by the doctor of medical psychology may at the same time serve the needs of many medical psychiatrists as well.

Consideration must also be given to the question of how medication can be prescribed during psychotherapy, for the patients of a doctor of medical psychology. Obviously a cooperative and understanding family physician could take care of simple sedation. But what of more uncertain "newer" experimental drugs? And what of the concurrent use of physiological and psychological methods in general? These questions create special problems for the non-medical psychotherapist. They demand of him both an open mind toward the organic therapies, and an inner security which will not feel threatened at the idea of organic adjuvants or short cuts. This also is one of the reasons why I emphasize the importance of exposure to patients with organic diseases early in the training of the doctor of psychological medicine. The use of organic adjuvants to psychotherapy will also require a fully cooperative rapport between the psychiatrist and medical psychologist. Yet here again we face a familiar problem; because an identical issue often arises between the medical analyst and the non-analytic psychiatrist over the introduction of organic therapies in the course of a psychoanalytic treatment.

Whether it has to do with theoretical conceptions concerning the interplay of organic and psychological factors in the etiology of the neuroses, or with the evaluation of somatic complaints which arise in the course of a psychotherapeutic experience, or with the concurrent use of physiological, pharmacological and psychological techniques either in research or in therapy, the coordination of the physician and psychologist would be mandatory both in the curriculum and under the law. This means that any license granted to a Doctor of Medical Psychology must specify clearly what roles he is allowed to play in all of these respects, and the specific nature of his relationship to physicians for these purposes.

Here then the essential safeguards consist of (1) the adequacy of the training and of the clinical experience of the Doctor of Medical Psychology; (2) the quality of his personal relationship to physicians; (3) the legal stipulations of his license with respect to his relationship to physicians; and (4) the nature of his affiliations with psychiatric hospitals to take care of emergencies.

VI. Licensing and Certification

Before there was such a thing as a system of legal licenses, physicians were trained to practice medicine. Licensing came only after the slow evolution of the medical curriculum. Usually the license has helped to safeguard the current level of education; although sometimes it has slowed up its advance. The general trend, however, has been upwards. More recently to the legal system of licensure by states has been added a system of post-graduate certification as medical specialists. This, however, has no legal standing. It is a system of extra-legal sanctions, imposed by doctors on doctors for the protection of patients in general. It is natural for clinical psychologists to attempt to emulate these two protective devices by establishing systems both of legal licensure and of extra-legal certification for clinical psychology. The purpose of these efforts has been good; namely, to eliminate charlatans and those who have had inadequate training, plus a further hope that the elimination of impostors and the untrained would give clinical psychologists a better chance to have their right to do psychodiagnosis and psychotherapy acknowledged by the medical profession.

Unfortunately many physicians have been frightened by these attempts to formalize and legalize the status of the clinical psychologist. They have looked upon licensure and certification as steps toward the granting of the very recognition against which they were fighting. These fears are excessive. Nevertheless, in fighting for licensure and certification before training plans were completed, before adequate training facilities were assured, and before a nucleus of competent teachers was trained, clinical psychology has in effect been by-passing its basic scientific and professional problems. Before establishing a system of licensure, clinical psychology must settle its quarrel with organized

medicine. The same point of strategy and timing applies to the campaign for certification.

In contrast, the plan which I am proposing would first establish a curriculum leading to a degree as a doctor of medical psychology, and then to a license to practice *not* medicine, but only psychodiagnosis and psychotherapy in a legally specified relationship to physicains and psychiatrists, the details of which would be prescribed in a *Psychological Practice Act* which would parallel closely the *Medical Practice Act*. Ultimately this should apply equally to physicians who practice psychodiagnosis and psychotherapy, as well as to practicing psychologists. In this way two potential abuses would be prevented by one law. Furthermore, this plan would require a close integration with physicians, whether the doctor of psychological medicine was practicing privately, in medical institutions, in social agencies, or in schools or courts. My contention is that if he is adequately trained in such a curriculum as is outlined here, it will be safe to license him to practice psychodiagnosis and psychotherapy *without psychiatric supervision:* but never apart from or independent of the medical profession. The critical point is that the licensing law must spell out every such necessary safeguarding procedure. Thereafter any doctor of medical psychology who violated the terms of his license would automatically be as guilty of malpractice as a physician would be who violated the Medical Practices Act. As such he would be subject to such disciplinary action as was prescribed by the law, such as fines, imprisonment, and the revocation of his license. Regulations of this nature hold physicians in line. There is no reason to assume that they would be less effective for the Doctor of Medical Psychology.

The graduates of this curriculum would also have to be examined by a joint board which will include clinical psychiatrists; and the subsequent licensure and their adherence to the legal stipulations of the license would be under the jurisdiction of this joint board. On passing these examinations, they will be given a limited license, which will indicate the precise nature of the obligatory alliance with physicians, to guard against somatogenic illness and psychosis. Such a license would also include the stipulation that if any man failed to maintain his constant alliance

with competent physicians his license would be revoked. In this way the patient would be exposed to no greater risks than if the therapist were a physician. Actually whenever psychotherapy is practiced by a physician who lacks appropriate training and for whose psychotherapeutic malpractice the Medical Practice Act, as formulated at present, holds no redress, the risks which a patient runs are much greater than this.

I would go so far as to say that under the scheme proposed no patient would be exposed to any risk that does not exist equally when he is in the care of a medically trained psychiatrist. For this statement there are several reasons. (1) The longer any psychiatrist has been practicing as a psychiatrist, the rustier will his medicine have become. Therefore, some danger of overlooking organic disease is ever present. Is a patient safer in the hands of a psychiatrist who practices the medicine of 1910, or of a Doctor of Medical Psychology, trained as outlined above, and associated with an internist of 1950? What is the honest and realistic answer to this question? What is more, it will be easy to check the activities of the doctor of medical psychology, so as to make sure that he is abiding by the law: whereas it is difficult to check up on the doctor of medicine, who can always claim that he has made his own physical examinations, even when these were of a cursory, inadequate, and antiquated nature.

VII. Difficulties, Objections, Timing and Strategy

Throughout the previous sections all relevant doubts and reservations about this plan have been carefully considered as they came up. Of the various objections the following were encountered most frequently: that we have not enough teachers; that the patients will not be adequately safeguarded; that it will jeopardize the close accord between medicine, psychiatry, and psychoanalysis which is one of the proud achievements of American medicine; that the interrelationship of biology and psychology in experimental work will be lost; that soon no medical psychiatrists will be left; and that it will open the door to all forms of unscrupulous and unscientific lay-healing.

None of these has been dismissed lightly: each has been answered in detail. Yet in considering their validity for non-medical psychotherapists, it is well to remember that even phy-

sicians who have gone through the complete medical curriculum and who may have spent years in the experimental laboratory find that once they are immersed in psychological problems their daily activities become quite different from whatever scientific work they had been doing previously. This does not mean, however, that the guiding influence of their previous organic training is lost, merely because it functions in the background rather than the forefront of their minds, in their preconscious rather than in their conscious processes. Certainly the non-medical psychodiagnostician and psychotherapist of the future, who like the psychiatrist has gone through rigorous training in the basic physical sciences, biology and clinical physiology, will also carry the imprint of this organic training into his psychological work. I fail to see, therefore how the invaluable symbiosis would be protected better by training in clinical medicine or clinical surgery than by the high quality of the basic scientific training. At the same time, since under this plan the doctor of medical psychology will have been exposed to the problems and reactions of patients with organic disease as well as psychiatric disease, both types of illness will be equally familiar to him. Therefore, we can count on his maintaining his awareness of the continuous interplay between organic and psychological processes: and also on his being neither indifferent to nor overanxious at any intercurrent somatic complaints or ailments among his patients.

As I have already indicated, under the proposed curriculum high standards of care will be served in still another way. One of the most important attributes of the wise physician is his sophistication, his skepticism, his watchful self-criticism, and his ever-present doubts about grandiose therapeutic claims, including especially his own. In medical school and hospital this spirit would be inculcated in the non-medical psychotherapists as well; and from these same medical traditions he will also acquire a dedication to the service of the sick, a sense of responsibility to their welfare and to the sick man's right to privacy, and a conviction that all sick people are equal before the therapist. These attributes of the physician at his best mark the atmosphere of our better schools of medical learning and research. Doctors and surgeons suffer from all human frailties, and do not always adhere to these ideals. Nevertheless the ideals are vital elements in a tradition which medicine does its best to sustain. Further-

more, this tradition has come down through generations, and cannot be acquired overnight. This is one of the many reasons why I emphasize the importance of conducting this training under the auspices of medical schools and teaching hospitals. It is only in their atmosphere and within their walls and in daily contact with doctors that the non-medical scientist will have an opportunity to acquire these same traditions.

Moreover, this close daily association cannot fail to cement a close relationship between the disciples of organic medicine and the disciples of medical psychology, closer probably than that which usually exists between internist and psychiatrist. Everyone knows how remote from their medical colleagues are the psychiatrists of most hospitals and medical schools. The psychiatric activities and the psychiatric staff as well are usually housed in quarters which are apart from the rest of the hospital. Frequently they eat in different dining halls, or cluster around their own tables, rarely rubbing shoulders in a day-to-day give-and-take with their surgical and medical confreres. In contrast to this unhappy tradition of separatism, students in a curriculum which would be integrated into the daily work of the young physician, both in pre-clinical training and on the organic wards, would almost certainly fare better than this. Consequently, this plan may well result in a closer integration of biology and dynamic psychology than has existed in the past. This could not be true if the training were undertaken solely in psychiatric hospitals, or in a purely academic atmosphere, far removed from any type of hospital and from any medical schools. When, however, the major training proceeds under the broad tent of organic medicine, the outcome should be a true marriage of the medical and non-medical forces.

But will the development of this new profession crowd the medical psychiatrist out of the picture, as some fear? To me this seems wholly unlikely. In every group of medical students there will always be some who extract from their medical studies a strong interest in psychological medicine. Indeed, I would anticipate that the influence on the climate of medical education of this paramedical group of medical psychologists working in medical schools, and their influence in teaching hospitals on the climate of ward practice will be such as to increase enormously the number of young doctors who will be drawn in this direc-

tion. This has been the experience of all who have practiced psychosomatic psychiatry on the wards of any general teaching hospital (9).

The only remaining objection is primarily a question of timing and strategy. Some colleagues fear that the timing of the proposal will jeopardize the acceptance of psychiatry and psychoanalysis as co-equals in the medical fraternity. These say frankly that although they concur in the desirability of the solution which I am proposing, even to the point of agreeing that it seems to be the *only* possible solution, they would prefer to leave the problem unsolved rather than support a plan which might alienate the official leaders of organized medicine and of the American Psychiatric Association.

In this connection these troubled colleagues point to the fact that organized medicine has been trying for many years to rid our community of unscrupulous and illegal charlatans, of fanatical if sincere therapeutic faddists and in general of uncritical folk in the therapeutic field. In the pursuit of these goals the AMA has conducted a vigorous drive against all forms of lay healing. Many of my psychiatric colleagues fear that if they were to support my proposals, they would seem to place psychiatry on the side of those lay healers which organized medicine has been driving out. I respect their caution; yet I cannot believe that a systematic and united campaign of education could not make clear to the public and to the leaders both of organized medicine and of official psychiatry the essential difference between an untrained fraud or fanatic and a Doctor of Medical Psychology who is the product of such a vigorous scientific curriculum as I have outlined.

Summary

It is evident that in any mature consideration of this issue neither the psychiatrist nor the clinical psychologist has any right to confuse valid, complex and troublesome issues, by making charges and countercharges about motives. Any mixed motives which are to be found on either side are irrelevant to the real issues which are at stake: to wit, the lack of enough trained men, the impossibility of filling this deficit from the ranks of medicine alone, and the paucity of teachers. Our purpose should

be rather to determine whether any particular plan is reasonable and then to protect it from misuse or abuse, so that it can be given a fair trial.

To me it seems obvious that the non-medical scientist can serve an important cultural, scientific and practical function in psychodiagnosis and in psychotherapy. It is equally obvious that any future non-medical doctor of medical psychology must not be a layman in the sense of being a hasty product of academic psychology, plus one or two years of testing patients, and a doctoral thesis based on some bit of "clinical" research. At the same time it seems equally incontrovertible that he must be first and foremost a scientist. As a scientist, however, he need not be a physician, provided that (1) by long exposure to medical atmosphere and influence he has been infused with all that is best in our medical traditions, and provided (2) that his practice is inseparably integrated with sound medical practice. If these stipulations are kept in mind, then it is both possible and essential to train psychodiagnosticians and psychotherapists from among those who are not trained to practice organic medicine. Under the proposed system each man could achieve the maturity which is necessary for competent research and practice in fewer years and at an earlier age than those who now go through a full training in medical psychiatry. Consequently, the same facilities will be able to turn out more fully trained specialists, each of whom in turn will have more years in which to practice his profession, simply because he has been able to save several years in training.

In the launching of any such program, the first step must be the training of a corps of experienced teachers. This will necessitate endowing a group of experienced psychiatrists to serve as teachers for an initial period of about ten years. At the same time a larger group of experienced clinical psychologists and other behavioral scientists must also be endowed to carry them through several years of training to become the new teachers in the new profession. Finally for the first trial decade this corps of newly trained teachers must be endowed again as they in turn start to train practitioners of the new profession.

Such a program would begin to yield significant results in twenty-five years. In fifty years it will have paid for itself many times over.

The entire proposal can be reduced to a series of concrete steps:

(1) Plan a training program leading to a Doctorate in Medical Psychology.

(2) Organize the curriculum under the joint auspices of a Medical School and of the Departments of Psychology, Sociology, Clinical Psychology and Cultural Anthropology of the same University.

(3) Plan a curriculum so that it will take at least six to eight years after graduation from college, the precise duration depending upon the previous equipment which the student brings.

(4) For the first five to ten years this curriculum will be reserved exclusively for the training of a corps of future teachers.

(5) This initial group of future teachers will be recruited from the ranks of experienced behavioral scientists, particularly those who have used their training in some form of psychodiagnosis and psychotherapy; *i.e.*, primarily individuals with long experience in clinical psychology, but also to include some psychiatric social workers and research workers in clinical psychological problems of all kinds.

(6) This group of future teachers will have to be endowed, first for the duration of their training; and secondly for approximately ten additional years, during which they in turn will start to train the future practitioners of the new profession.

(7) These future practitioners of the new profession will be accepted for this professional training only after graduating from college with certain minimal equipment in the basic general sciences and in behavioral sciences.

(8) *The curriculum will consist of:*

(a) The basic medical sciences.

(b) Experiences as a psychological interne or externe, first on organic wards and out-patient departments, and then on psychiatric wards and out-patient departments. This experience will involve direct service as an aid, in history-taking, and in psychological testing.

(c) Experience as a medical social worker, first among patients with organic illnesses, and then among patients with psychiatric illnesses.

(d) More advanced training and experience in clinical psychology, for diagnostic purposes and also in the use of psychological test instruments in research.

(e) Experience with group psychology: first as a patient, and then as a leader of the group.

(f) Experience in individual psychotherapy: first as a patient, and then as a therapist.

(g) Psychoanalytic training: first as a patient, then as an analyst under supervision, and later independently with adults and children.

(9) After passing examinations given by the university medical school which had conducted the curriculum, the degree of Doctor of Medical Psychology would be granted at graduation.

(10) A legal license to practice would then be granted by State and/or National Boards of Medical Psychology after the student passes suitable examinations. These would be administered by the licensing board, which would be composed of doctors of medical psychology, psychiatrists, and representatives of the faculty in the basic medical, biological sciences.

(11) This license would be to practice psychodiagnosis and psychotherapy independently of direct organizational or individual supervision; *except that detailed and mandatory regulations* would be specified in the license, requiring arrangements for complete medical examinations when a patient is first seen and periodically throughout the course of treatment. The license would also carry the mandatory stipulation that for every case the Doctor of Medical Psychology must arrange to have a certified physician supervise general health and also the use of any medications during the course of the treatment.

(12) The necessity for emergency hospitalization would be dealt with in one of several ways: (a) through a special mixed hospitalizing board; (b) through mandatory liaison with an individual certified psychiatrist; or (c) by granting to the doctor of clinical psychology some right as a psychiatrist to certify as to the necessity for immediate hospitalization, at least for purposes of emergency protection and to determine whether or not commitment is necessary.

(13) Ultimately the implementation of Sections 11, 12 and 13 will require the passage of a *Psychological Practices Act*, based on and paralleling existing *Medical Practices Acts*.

(14) In the meantime, a leading university and medical school should undertake a pilot test of the entire proposal.

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CHAPTER VI

PROBLEMS OF INTERDISCIPLINARY RESEARCH IN THE CARE OF THE CHRONICALLY ILL

QUESTIONS CONFRONTING THE PSYCHOLOGIST INVESTIGATING CHRONIC ILLNESS

BEATRIX COBB

The last decade has seen a shift of interest in medical practice toward the understanding of the organism as a whole. During this period, the historical development of psychophysiological investigation has dramatized both the possibilities and limitations of present methods. The challenge of possibilities has set off a wave of intensive explorations of numerous diseases heretofore not connected routinely with psychological services and research. As the various chronic diseases have been studied in light of psychological implications, a new field of contribution seems to open for the psychologist in the treatment-research team. This is a two-fold contribution consisting of additional effort toward insuring emotional comfort to the chronically ill and supplementing medical knowledge about the patient as a person through research.

In such an atmosphere, it was inevitable that a disease so weighted with emotional impact as is cancer should be brought into focus. Psychologists have been members of the medical team in several institutions dedicated to treatment and cure of cancer for the past four years. As a member of one of those teams, composed of scientists, epidemiologists, physicians, and psychologists, it has been encouraging to observe the scope of possible contribution for the psychologist in such a setting.

Three specific areas of service and research have been found profitable for psychological endeavors at M. D. Anderson Hospital for Cancer Research, The University of Texas. First, the psychological response of the patient to the disease itself is being studied. Second, the study of the psychological impact of treatment, which includes hospitalization, reaction to the doctor, and such things as mutilating surgery, chemotherapy, and radiation,

is yielding interesting information. Third, research which has to do with the psychophysiological basis of the disease is being done in conjunction with the total research team. It is too early to report results. Questions which have arisen and which are applicable to other chronic illnesses may be of value to report.

What is the nature of the contribution of the psychologist as a member of the treatment team in an institution caring for the chronically ill? Some wise man has said that it is not enough to make the patient comfortable, he must be comforted as well. In cancer, with its frightening connotations for most, the psychologist is able to give the patient comfort through supportive therapy. As this supportive therapy progresses, such questions as the following arise. What is the psychological threat of cancer? What is the meaning of cancer physiologically to the patient? He may envision it as an infectious sore, or as a tarantula-like entity with long black tentacles pushing relentlessly throughout his body. How do these preconceived notions of cancer contribute to his psychological reaction to the disease? Why does one person seek medical attention immediately when cancer is threatened, while another may delay for months, or detour to non-medical "quack" practitioners for treatment? Why do people refuse prescribed treatment? What are the patient's needs in terms of potential for recovery or malingering toward chronic invalidism? The answers to these questions are of importance in the understanding and management of the patient. The psychologist is specifically trained to secure and interpret such information.

In the service area, too, lie the questions surrounding the effectiveness of individual therapy versus group therapy. Here again the psychologist is capable of rendering service contributing toward emotional comfort of the patient and at the same time test the comparative value of the two methods.

It is meaningful to the physician to know his own psychological response to the disease. How does his reaction to cancer, for instance, find reflection in his handling of the patient? How does he feel about the patient as a person? Does the counter-transference, which is inevitable in instances where the disease or the personality of the patient creates a negative counter-transference on the part of the physician, find reflection in the progress of the patient? A recent study indicates that 84 per cent of the

patient's concern about his physician is centered around interpersonal relationships. The cancer patients investigated desired a doctor with a warm, friendly attitude, a doctor who would talk to them and listen as they expressed their troubles, one who identified with the patient and recognized him as a person. Only 16 per cent of the attributes of the ideal doctor concerned professional skills. What would a negative counter-transference do to such patients? How aware are doctors of this most important of all his instruments—himself? How familiar is he with himself as a person, with his basic compulsions, his pre-conscious motivations in medicine?

These and many other questions have been raised concerning the psychological response of the patient to the disease and to his doctor. Many more have been brought to attention concerning psychological response of the patient to hospitalization and to treatment. What are the effects of hospitalization on the patient? It has been observed that a group of patients coming into the hospital for metabolic studies have a consistent regression of disease during the week of controlled study wherein no treatment is given. They seem to get better simply because they have presented themselves for treatment; they relax and place their anxieties to some measure in the hands of the physicians, and the cancer actually regresses a bit. Why? This phenomenon has been labeled "psychosoteria" by a physician in the institution and systematic investigation both chemically and psychologically is being carried out.

What are the effects of surgery on a patient? What is the source of the basic fear of the knife so prevalent among patients seen in cancer treatment centers? What are the psychological repercussions of the loss of a breast to a young woman? How do women react to the threat of complete hysterectomy? What is the psychological impact of mutilating facial surgery and the process of plastic surgery? What happens to the body image when it must be destroyed and then built back by a surgeon who is not aware of the original? Would careful psychological studies assist the busy plastic surgeon in making less traumatic the changes necessary?

What about the man who loses his voice through drastic surgery required in cancer? What is the psychological component

of this physical loss? How may the psychologist assist in rehabilitation during the course of voice restoral through esophogeal speech?

There are known personality changes subsequent to hormonal therapy. What does this do to the patient psychologically? How does the woman who suddenly develops a deep bass voice and hirsuitism feel about herself and others? How may adaptations be effected with a minimum of trauma to the patient?

The foregoing are only a few of the crucial problems confronting patients in which the psychologists may make a meaningful contribution. The training of the psychologist also makes him a productive member of the medical research team. Much is being written and thought recently concerning the psychophysiological basis of diseases. Many questions arise for investigation in the study of cancer. Most of them are directly transferable to any chronic illness. What do we mean by predisposition to a disease? Why do some people who are under severe stress for a long period of time become psychotic, while others become victims of psychosomatic illnesses? Are there physiological fixation points at a primitive level similar to those points of psychological fixation we premise? Careful investigations of psychic histories along with physiological developmental studies may prove profitable in answering these questions. Would a systematic exploration of the emotional climate in which specific diseases develop assist in uncovering psycho-physiochemical patterns of interaction?

Is there an organ selection mechanism in chronic illness? Doctor Margaret W. Gerard has found hints as to possible biographical experiences which may play roles in the sensitizing of different organs, or organ systems, so that the organs break down in later life to exhibit physical pathology. How may we assist in testing this hypothesis in chronic illness? Is there a trigger mechanism which precipitates the chronic disease? If so, is it a psychological or a physiological factor?

Much is being thought and discussed about "host resistance" in cancer. Is there a host resistance factor in all chronic illness? How does it happen that two patients in the same stage of the same disease and with similar constitutional endowments follow completely divergent courses toward death? One may resist the onslaught of the disease and linger for months, even years. The

other may die in days. Why? Are there psychological factors operating which might throw light upon this phenomena?

Possible contributions of the psychologist in a medical, as compared with a psychiatric, setting are varied. The field of psychological service in a non-psychiatric institution is new and undeveloped. In one medical institution dedicated to treatment and research in cancer, three phases of endeavor have been found profitable for psychological techniques. The psychological response of the patient to the disease, reactions to treatment and the hospital environment, and psychophysiological research provide many possibilities for meaningful contributions from the psychologist.

Perhaps the greatest challenge to the psychologist in this new situation is that of demonstrating effective teamwork attitudes and relationships. The busy physician often does not know just what the psychologist's skill prepares him to do in the medical setting. This educational need must be filled on a developmental service-when-indicated scale so that horizons may be widened for both physician and psychologist and the team effort synchronized. To do this the psychologist must be well aware that his success in a medical setting is dependent upon the ready acceptance and use of his skills by the physician of the team.

THE COMPLEX SCIENTIFIC CHALLENGE IN CHRONIC ILLNESS

LAWRENCE S. KUBIE

One of the significant purposes of such a conference as this is to rescue the chronically ill from the uninstructed and confused ministrations of the do-gooder. This does not mean that simple social aids are not urgently needed. Nor does it mean that a more intelligent use of all available resources will not make a considerable difference in the degree of disablement, of disability, and of dependency from which the chronically ill suffer, and consequently a difference in the burden which they place on their families and on the community. All of this is of increasing importance today, while medicine is in transit through a phase

in which it can keep people alive almost indefinitely, without making them well.

Yet from the point of view of science, this is good but not enough. There are complex and subtle problems that cluster around the chronically ill; and a conference which is made up of scientists, should accept the challenge of chronic illness in its full complexity and not in simplified terms. This is necessary, even if it makes it seem at times as though we were placing obstacles in our own path. Actually I do not believe we will be placing them there. I believe that the obstacles are there; and that we cannot remove them by ignoring their existence. I will mention a few of these complications here, and others through the course of our deliberations.

In the first place, there is the effect of longevity itself on chronicity of organic illness; *e.g.*, on its age distribution, the duration of dependency, the interrelating of chronic organic illness and chronic neurosis. In this connection it must be recalled that in addition to the longevity of the older age period (*i.e.*, over 60), there is an increasing percentage of population under 17. This too constitutes a challenge to those who study chronic disease; because inevitably it brings up the complex problems of prevention and cure of neuroses in people, young and old, with chronic organic ailments.

This last always presents special complications. In some people organic illness reactivates neurosis. In others it acts as a mask for neurosis, or alters its form. Sometimes chronic illness masks actual psychosis, which erupts when the chronic illness is alleviated.

Nor should we forget that we can have a chronic physical disablement linked either to a chronic neurosis or to an acute neurosis; or alternatively an acute physical disablement linked either to acute or to chronic neuroses. Again I stress the two fundamental forms of this inter-dependence: *i.e.*, the man whose neurotic problems are activated by organic disease, and who is thrown into panic, depression, tension, rage, or agitation when he faces organic illness; as over and against the individual for whom organic ailment is a haven of rest and retreat, much as pregnancy so often serves, and precisely as hysterical symptoms always functions. This type of man achieves through chronic illness the "belle indifference" of the classical conversion hys-

teria; and manifests his emotional problems only if and when he recovers physically.

This is only one example of the complex, subtle interrelationships between organic and neurotic factors in chronic disease. Yet alone they would justify such a conference as this for the consideration of the role of the psychiatrist and of the clinical psychologist and of the psychiatric social worker in conjunction with the internist, surgeon, physiotherapist, occupational therapist and vocational advisor in the care of chronic disease. None of these can function with maximal effectiveness if the role of the neurosis is left out of account.

THE ROLE OF THE PATIENT AND PATIENT-GROUPS IN THE STUDY OF CHRONIC ILLNESS

HARRIS B. PECK

I suspect that I have a somewhat unique role in this meeting because so much of my prior training, experience and interest seems to be diametrically opposed to the matters which we are called together to examine. I have had relatively little to do with somatic illness and a good deal of my recent psychiatric work has been concerned with acute rather than chronic pathological processes. My interests tend to center more around the maintenance of health rather than the treatment of illness. We know that concepts of health and illness, to have any meaning at all, must be related to some specific aspect of the organism's functioning. Even among the chronically ill, generally some aspect of the individual's person remains relatively intact and may play as significant a role in determining the course of the illness as the specific pathology. Thus, in the study of chronic illness, it may be well to pay some attention to the processes by which health is maintained; and especially to examine the psychological components in such processes.

Some seemingly healthy people seem to collapse into almost total deterioration following what appears to be a relatively minor affliction. On the other hand, some individuals suffering from chronic disturbances which affect sizeable segments of their persons nevertheless manage to preserve intact whole areas of functioning. It would seem of some importance to learn

more about the conditions and dynamics involved in such achievements. Perhaps it would provide clues that might be useful to those who are trying to bring about similar happenings in the treatment of the chronically ill. An important by-product of such studies is the information that might be obtained regarding the maintenance of health and the prevention of illness.

As one who has been very much concerned with the theoretical, methodological and technical problems of a public health psychiatry, I have been very much impressed by the crucial role of group processes both in matters affecting the maintenance of health and in those affecting the course of prolonged illness. The study of such processes draws attention to an aspect of the problem which I think may not have received sufficient attention at this meeting, namely, the role of the patient himself in altering the course of his illness. As in dealing with other overwhelming problems, people must rely not only on their own resources but on those strengths which are derived from their relationships with other people. Group psychotherapy in the earliest days of its history turned its attention to work with certain of the chronic physical illnesses such as tuberculosis. In the more recent period, we have seen a number of non-clinically oriented developments in the utilization of the group process in chronic illnesses. There has been a rapid acceleration in the growth of many types of patient organizations in such fields as Cerebral Palsy, the mentally retarded and Multiple Sclerosis. As random and various as are many of these developments, they present a potential field for exploration into such matters as the great problem of dependency which is so intimately associated with the difficulties of many chronically ill patients. In certain of these groups we see patients themselves actively engaged in grappling with the very forces which seem to be thrusting them into chronicity. More than one physician struggling with difficult practical problems has found that under some conditions, patients may become remarkably effective allies.

In experience in working with the parents of schizophrenic children at Bellevue Hospital, I was very much impressed by the role of the group in assisting the physician to mobilize the strengths within the family in the service of the patient's treatment. Here we see an illustration of the way in which we can further extend our techniques for attacking the illness, not only

with our own therapeutic devices but also with all of the potential weapons that can be drawn from the patient and those who are meaningfully engaged with him.

Work with groups of patients in addition to being a therapeutic measure may also yield useful clues for research. In our Bellevue groups, for example, we initially set out to try and assist those adults who were important in the life of the schizophrenic child. In the course of doing so, however, we discovered a good deal that we had not previously known regarding the patterns of family interaction common to this illness. Some of this data had been entirely missed in the course of individual interviews with patients and their families and only became apparent through our observations of the configurations within the group. This suggests that work with groups of patients may be an additional research instrument in our study of the dynamics of chronic illness.

The study of many of these matters is still quite embryonic and one should not expect that any great inventions will suddenly emerge as solutions to the problems with which we are here concerned. I say this with no tinge of pessimism but rather with a conviction that many of the kinds of processes which seem to bear relevantly on the psychological problems of the chronically ill occur at many levels of integration. Thus, we will have to study them using such diverse instruments as those provided by psychoanalysis, social and clinical psychology, physical medicine and anthropology. One of our most difficult methodological problems will be to find research settings in which there is some prospect of obtaining data which lends itself to integration and synthesis of what are often the varying manifestations of a single process. An important contribution of this conference might be in the planning of settings for this kind of research.

REPORT OF STUDY GROUP ON RESEARCH

Doctor Shorr opened the session by asking for a brief statement from each participant regarding background, current interests, and expectations from the current joint endeavor. Preliminary discussion led to the adoption of a tentative orientation that we

(1) envision a few programs and areas of research relative to the coordination of effort in the care of the chronically ill; (2) sketch some general methods by means of which such programs of research might be implemented; and (3) consider the relations of the clinical psychologist to these endeavors.

Tentative frameworks within which discussion might proceed were at this point suggested. One participant raised a series of questions typified by the following: "Is there a basic personality pattern that goes along with rheumatoid arthritis?"; "Is there a basic personality pattern that pre-exists in an individual and renders him more vulnerable to certain types of disease than to others?" A second participant was of the opinion that to attempt to deal with specific diseases might prove an endless and uneconomic pursuit. He suggested that in place of *specific diseases*, *prognosis* might more fruitfully be made the frame of reference. He proposed that inquiry be directed at the psychological make-up of individuals suffering from (a) diseases in regard to which a happy ending may confidently be expected; (b) diseases in which the expectancy of the outcome is good but necessarily carries with it a moderate measure of uncertainty; (c) diseases in which the physician and patient must accept a *status quo*, in recognition of the improbability of improvement, or even a degree of deterioration which, while not rapid, must nevertheless be expected; and (d) diseases in which a tragic outcome appears inevitable. A third participant suggested that we deal with personality aberrations as they relate to (a) the relatively static disease pictures on the one hand and (b) the relatively changing clinical pictures on the other.

With minor reservations, the consensus of the group was that progress in research might be seriously compromised by adopting formulations bearing on specific diseases. It was felt that the most rewarding progress might be made through the projection of wide, even seemingly disparate, explorations in which basic principles and common denominators may be expected to emerge. Later, armed with certain broad principles, inquiries bearing on narrower areas of disease and even specific disease entities having special interests for special groups might be pursued.

At this point Doctor Huston expressed the conviction that *methodology* cannot legitimately be separated from investigation

and that, since investigation proves rewarding in proportion to the accuracy of the tools for making observations, a continuous endeavor must be made to refine the relatively crude psychological tools at present available. This led to a rather extended discussion on ideal circumstances for the conduct of investigations. Mrs. Beatrice Wright indicated that any present attempt to set up and implement an "ideal" research program involving the human infant, child and adult and extending over the course of many years might prove discouragingly formidable. She urged that investigators must be realistically prepared to do whatever they are able, employing whatever tools are now available. She felt that a start can and should be made on a number of problems, even when conditions for conducting the inquiry are less than ideal. She contended that past experience in research provides reasonable assurance that better observations and better tools appear as inquiries are pursued. The group then agreed that once a felt need had become sufficiently well defined to make a formal attack possible, presently available techniques should be exploited to the full in making extended observations and recording them as accurately as possible. This much appeared to be wholly realizable.

A number of specific formulations now emerged from the general discussion as summarized above.

1. The question was raised, "In what kind of a framework might we begin to work?" Doctor Shorr proposed the answer, "An out-patient department of a hospital which furnishes cases of the general type represented by Graves Disease." It was Doctor Shorr's opinion that such cases offer unusual opportunities to study the personality make-up of the individual (a) when he is passing through the acute and subacute stages of his organic derangement; (b) when his derangement has been corrected to his satisfaction and that of his professional attendants; and (c) if and when he swings into an antithetic state (as when the hyperthyroid individual passes through euthyroidism and swings over to hypothyroidism). The broad principle under inquiry here would be to ascertain how dependent or independent personality characteristics of the patient may be of the endocrinopathic disturbance.

2. A suggestion was made that the impact of diseases on personality and *vice versa* might be implemented by studying the well known "reversible diseases," especially those classified as acute. An example given was infectious mononucleosis. Some discussion followed and against the recommendation it was urged that the patients most seriously in need of help and of the development of sound hygienic formulations are those suffering from chronic and intractable diseases. It was recommended that the latter diseases merited first attention. By way of reconciling these polar points of view, Doctor Meyers drew attention to the possibility of studying certain clinical conditions, *e.g.*, decerebration, which may be either acute and reversible or chronic and intractable and all intermediary shades. Such conditions, being manifestations of physiological disturbances, are invariant to etiology and pathology. This circumstance provides a considerable advantage, since the yardstick by which the condition is recognized is fashioned from certain easily recognizable overt manifestations. Patients with such conditions exhibit a wide range of recoverability from nil to complete. Correspondingly, a wide range of neurologic, psychologic, humoral and systemic changes lie open to inquiry. In theory, there would appear to be no limit to the number of disciplines that might profitably cooperate in studying patients of this type.

3. Doctor Kohn proposed a study of the psychologic makeup of the healthy, elderly individual approaching death and a comparison of such findings with those arising from similar study of the unhealthy, elderly individual approaching death. In the course of the subsequent discussion, Doctor Cobb indicated that studies of this sort have already been begun at the M. D. Anderson Research Center. Some of the difficulties of carrying out intensive and extensive psychologic studies on such patients were pointed up. This led to a consideration of the possible limitations of psychologic study in such conditions. It was the expressed opinion of Doctor Harrower that there is probably no clinical circumstance so difficult but that some useful psychological evaluation might be reached. She asserted that the problems of the psychologist appear to be far less a question as to which techniques should be used than of getting the clinician meaningly

to advise the psychologist as to what his problem is. She emphasized that, whether for the individual or group, it is of the utmost importance that the psychologist be advised as precisely as possible concerning the information desired. The more specific the formulation of the question, the greater the likelihood that the psychologist will provide specific answers for the investigation.

4. Doctor Beatrice Wright indicated that at present it should prove easy to fit the psychologist into three classes of clinical investigation. First, the psychologist may help clarify problems of communication between the patient and those, such as the physician, attempting to help him; second, the phychologist might play a useful role in public education as it bears on chronic disease; third, the psychologist might study in a variety of circumstances the question as to which individual of a team might most advantageously be assigned the job of working closely with the patient.

Amplifying on the first suggestion, Doctor Wright felt that one might start with a verbatim recording of the every-day conversations between physicians and their patients. Later, the psychologist might interview the patient and doctor, singly and together. With the material thus accumulated, the psychologist might then attempt to identify areas of misunderstanding and recognize the kinds of persons most likely to misperceive. At this point, Doctor Cobb reported that a project of similar type is already under way at the M. D. Anderson Hospital. This makes use of a psychological tool called "the ideology test." This consists in requiring the patient to list eight things he feels the doctor *should* do, say or think and eight he *should* not do, say or think. The patient is then requested to draw up a list of characteristics which he believes to be those of "the ideal doctor." Doctor Wright was concerned with the question, "What kinds of information regarding physical and mental illness should be disseminated through public channels of information and how can it be disseminated with the least amount of trauma to the individual sufferer?" She drew attention to the propaganda employed by agencies interested in heart disease, venereal disease, tuberculosis, poliomyelitis, arthritis, multiple sclerosis, etc. Amplifying on her third suggestion, Doctor Wright felt we might profitably study the question as to when and under what circumstances the patient himself and/or his

family might be brought into professional conferences bearing on his problem. Discussion led to general agreement that in some instances the patient might quite justifiably be regarded as an expert on himself and that his entrance into deliberations concerning the management of his case might prove highly profitable. Doctor Wright suggested that verbatim recording can be made a useful means of carrying out certain studies. The first such experiments might be made in cases in which the professional staff feel that the patient's reactions can confidently be predicted. Detailed study of such recordings might then provide information from which new jumps might be made in relation to patients whose reactions are less capable of confident prediction.

5. Doctor Peck suggested that a series of studies might be projected in the hope of defining the nature of chronic disease, *e.g.*, "What makes some acute physiological disturbances become chronic 'illnesses'?"; and, "What enables some severely chronically ill patients to function with relative effectiveness?" Doctor Kohn, thinking along more general lines, asked the question, "What is the emotional climate in which chronic illness develops?"

6. Doctor Kohn noted that elderly people are being sent in increasing numbers by their families to convalescent homes. Such action represents the intermediate choice between retaining the patient in his home situation and committing him to a state hospital. Doctor Kohn felt that the psychological reactions of elderly people relegated to such convalescent homes might be made a profitable line of inquiry. In this connection, Doctor Huston considered that one might study the broad problem of the clinical signs of institutionalization, paternalism and dependency which are so often grafted onto the primary disease for which the patient is sent to any institution.

7. Doctor Shorr urged an inquiry into the variety of ductless gland disturbances as they relate to chronic disability and personality alterations.

8. Doctor Wright proposed that an intensive study be made of the relation between the structure of a research team and its effectiveness in solving problems relative to chronic disease. In this connection, Doctor Meyers cited experimental studies of

group dynamicists bearing on this problem and indicated that such studies are leading to the development of a new "mathematics of matrices." Some of these studies were briefly reviewed and the possibilities of applying such techniques to the specific problems of chronic illness were discussed.

9. A recommendation was made that psychological tests and personality evaluations might be more extensively employed with medical students. The results of these studies might be correlated with the subsequent performances of the subject during and beyond medical school.

10. Doctor Shorr now proposed that an effort be made to draw up an operational list of those assets that might be considered actualy or potentially "positive" for individuals suffering from chronic illness. As an example of such, he cited the lack of necessity for conforming to certain social and familial amenities. Doctor Shorr's notion was that such assets, once clearly identified, might be more fully exploited by the investigator and therapist than has been possible up to the present.

11. In an over-all review of the group's deliberations, it seemed to Doctor Meyers that our immediate task entails a study of the broad picture of chronic disease rather than of specific diseases and individuals suffering therefrom. The early steps involved in carrying out research in this difficult area consist in defining the problem and in framing operationally meaningful questions. The next steps consist in making dependable observations and compiling accurate records. Going beyond these early steps, it becomes necessary to arrange, order and classify the acquired data with a view to arriving at tentative hypotheses, principles and theories. The practical question arises at this point, "What can the investigator do to facilitate his efforts at effective classifying and hypothesizing?" Doctor Meyers recommended holding hands as closely as possible with colleagues in a variety of other areas of human endeavor, not only in the scientific disciplines more obviously related to our interests in chronic disease, but in all the sciences, arts and humanities. The deliberate maintenance of such contacts often discloses relationships in the data of one's necessarily circumscribed inquiry which might not otherwise become apparent. It has the additional virtues of unifying human endeavor and of fulfilling the requirements of Occam's

Principle of Parsimony. Such has paid large dividends in the past and promises more in the future.

Ephraim Shorr, *Chairman;* Russell Meyers, *Recorder;* Beatrix Cobb, Paul E. Huston, William C. Kuzell, Lawrence Kohn, Harris B. Peck, and Beatrice A. Wright.

DISCUSSION OF STUDY GROUP REPORT ON RESEARCH

Shorr: In all of our discussions we failed to describe what a medico-psychological team is, because we felt that what a team might be would emerge from the discussions in this conference.

Robbins: The committee considered the kinds of information that might wisely be disseminated to the public. County Medical Societies are attempting to ingratiate themselves with the public by inviting local physicians to get up TV programs and discuss particular diseases, including chronic ones. These are not particularly successful.

Leake: We have found on the contrary, that simple discussions of what doctors and others who work with them, are trying to do in caring for sick people, goes over well, either in TV or on the radio. There seems to be real evidence of understanding in this part of the country of what good medical care may do in chronic disease. This helps in obtaining support.

Peck: Chronically ill patients in rural areas are often able to undertake much that can't be done in big medical centers. Medical and psychological teamwork in such cases must include members of the family. We should sometimes look to them for clues and ideas, and include them in our research effort.

Leake: Educational efforts in connection with chronic disease in large metropolitan areas may run into trouble because organizations specifically concerned with such chronic diseases may feel that attempts are being made to invade their areas of interest. Conference discussion may help in breaking down such barriers in getting such information around regarding the care of the chronically ill. The public is really interested in the care of the chronically ill because most families have someone with whom they are directly concerned who is chronically ill and about whom they are worried.

Shorr: In the frenzy of fund raising during the past fifteen years for research and other funds in connection with specific chronic diseases, each particular chronic disease is painted in highly dramatic tones. Perhaps this results from the competition between the organizations for support.

Kubie: In any effort at health education for the public we run up against the situation that many myths regarding health and disease exist. People need these myths and cling to them, and they are not always banished by supplementing them by fact. This is not an argument against attempts at health education, but it is an argument that all who are concerned with health education should keep in mind the resistances which occur. Human beings have phobias and counter-phobias. They are sometimes unduly frightened,

and sometimes unduly unfrightened. Some people go through epidemics of diseases with a spurious feeling that they can't catch it. On the other hand there are many people who suffer from the notion that they will inevitably succumb to a lingering chronic illness. In any health education effort one will not touch the person who thinks it can't happen to him, and on the other hand one may cause panic among those who fear the worst.

Garrett: It is very important that those who undertake health education be careful in the selection of material so as not to unduly frighten those who are concerned about the possibility of coming down with chronic illness. On the other hand it is equally important that hope be maintained that reasonable adjustments can be made in the face of chronic illness. New information derived from research in respect to chronic disorders should be carefully handled for the benefit of the public. I am sure material derived from the University sources is usually carefully edited. It is the indiscriminate sensational stories which appear in some of the flamboyant national magazines that tend to cause unjustified hope, and thus subsequent danger of depression when the false hopes subside. Perhaps it would be wise to maintain emphasis on the importance of continued adjustment and adaptation and rehabilitation, rather than on chronic disease itself.

Seidenfeld: As one who has had to deal with problems of health education under many aspects, I have come to the conclusion that it is extremely difficult to arrive at any satisfactory way of developing real health education. Motivation is important. There are blocks of barriers and resistances, but usually we suffer from the enthusiastic writer, who visits a little while in a hospital, and then writes a sensational article about wonder drugs, wonder treatments, or miraculous new procedures. On the other hand a well motivated writer can help greatly by discussing research and development in relation to the care of the chronically ill by employing lay terminology. In our own literature regarding polio, I make extensive use of the committee procedure. Material is sent to those who are expert in their fields for criticisms and comments. When we do not have unanimity of opinion, we may frankly admit that there is difference of opinion. Results of research may be interpreted in different ways. In regard to adjustment to chronic illness, a public education effort might well be directed toward the understanding of the extreme complexity of the problem.

Holsopple: Research in the psychology of chronic illness seems essential to me because it grows out of a need that is so widely felt, to know something helpful and important that we do not now know. It does not seem that psychiatrists and psychologists are prepared to do the research studies on chronic disorder that needs to be done unless they begin to find out what are the needs of the people who are dealing with the problem. It is necessary to know what the attitudes are among the public, among those who have chronic disease, and among those who are caring for the chronically ill. Some answers may perhaps be found through opinion research, and attitude research.

Fremont-Smith: There is need to appreciate carefully what Doctor Kubie has said about myths, which are the hidden resistances, in connection with

attitudes on any matter. In the care of the chronically ill the clinical psychologist must learn what is expected by the internist or psychiatrist. The general practitioners and the specialists concerned in chronic illness often do not know how to begin to think about the problem. What is needed from the clinical psychologist in teamwork in connection with caring for the chronically ill? What can the clinical psychologist give to the team effort? Great contributions can be made by various disciplines in the adequate care of the chronically ill. Perhaps the clinical psychologists can bind the team together.

CHAPTER VII

GENERAL DISCUSSION

This discussion followed the exposition by Doctor Russell Meyers of selective responses to intra and extra organic environment.

May: I would like to share a curious "dejavu" reaction which I have had during much of our discussion. It has dawned on me that many of the problems concerning human ills which we have been discussing have been in different terms the very problems that many philosophers in various historical ages have pondered. When Dr. Berry, for example, points out that every man over forty enters into some form or other of chronic illness, he is talking about what many philosophers have called the *finiteness* of man. We get old and we run down physically and we die, no matter what we do or think about it. "Man's years are like the grass; they withereth," as the Psalmist phrased it. Some of these philosophers, furthermore, have strongly emphasized that it is very important for man's mental and spiritual well-being that he be able to realize and accept his finiteness.

It also dawned upon me, somewhat to my surprise, that many of these philosophers were not nearly as negative and pessimistic as we have been here in our conference. They held that old age should bring an invaluable kind of wisdom. Certainly they were referring to something more than merely the fact that old people have had time to accumulate more facts. They meant rather that if one can accept his finiteness, if he can face the fact that we all are prey to infirmity and grow old and die, then he has achieved a dimension of wisdom much more important to him than the quantitative years he has lost. This realization yields a qualitative ethical, spiritual, and emotional value. It seems to me that our error arises from a kind of perfectionistic assumption, which is typical of our modern progress-worshiping society. May not the philosophers of previous ages be wiser than we when they point out that the inevitability of old age, infirmity, and death need not be treated as pessimistically as we have tended to do?

Fremont-Smith: We do not have to accept as fact that the rate of aging is the same all over the body. Some parts of our body may run down while other parts may continue to develop. If we can maintain the functioning of the central nervous system, there may always remain some comfort even in extreme old age.

Ham: In connection with our discussion on semantics, it is important always to recognize the common symbols that may be emotionally loaded. Often these may be translated into terms that are less emotionally charged and thus help promote communication between the chronically sick person and those who are trying to help such a person.

One of the goals of modern treatment is the removal of pathological road-blocks with which people are born. In medical teamwork with psychologists another goal could well be the removal of psychological road-blocks which we

all may acquire. Here it is important to realize the dangers we can get into by certain cultural values to which we may be expected to conform. We tend for example to take great pride in our technological proficiency, but fail to realize how primitive we are in social understanding and social coordination.

Peck: I was bothered this morning when several of us were speaking of the possible good that might come from being chronically ill. Increased effectiveness and understanding were mentioned. My first feeling was that there is something wrong in this point of view. Yet on thinking of some of the cases I have encountered I realize that I have seen chronic illness bring about crucial changes so that those patients who were suffering from such chronic disorders start doing things they might never otherwise have done. Some of the advances that we frequently see in patients following a chronic illness are not necessarily related to the illness itself, but to the release of potential interests and abilities which were there all the time, but which under the pressure of ordinary living may have been blocked off from growth. Many of us have been impressed by the remarkable success of programs for mentally defective children. Frequently in illness people have an opportunity to think deeply about themselves and their relations to others, and thus to learn some of the matters that may help them in getting along better with other people. Somehow we must learn to take advantage of this opportunity in connection with chronic illness.

Kubie: A patient's "acceptance" of a mutilation, deformity, or any chronic ailment depends upon his pre-morbid attitude to his own body image and, indeed, to his own sex. Physical handicaps have multiple idiosyncratic meanings to each individual on conscious, pre-conscious, and unconscious levels. The socio-economic meanings of illness are relatively constant and predictable for people generally; but the impact of such meanings on each individual must vary not only with varying cultural attitudes, but with the varied constellations of unconscious attitudes in each patient. This is why one patient may actually welcome marked disablement and mutilation; while another will be completely undone by a relatively slight disfigurement.

Brown: For the past six years we have been developing a plan for intermittent hospitalization for the chronically ill. This has many opportunities for effective development and particularly for promotion of teamwork between patient, relatives and friends, and all the complicated personnel who may be involved in hospitalization including internists, psychiatrists, and psychologists. We all know the ordinary hospital for the chronically ill, loaded with patients with nothing but hit or miss perfunctory therapy and what really is merely custodial care.

Usually chronically ill patients, feeling socially rejected in such a situation, irritatedly accept the proposition of remaining there for the rest of their lives. On the other hand other patients who may be fully aware of the general situation in their own cases, and seeking security, may have in mind staying in such a hospital the rest of their lives, perfectly content with the routine activities of writing friends, having visits from Gray Ladies or other groups,

with movies occasionally, and with the usual routine of eating, playing games with each other, and sleeping.

The Veterans Administration Hospitals are usually good-looking institutions, with pleasant atmospheres, and with circumstances and conditions conducive to patients willing to accept the security of the situation in remaining under such conditions indefinitely. Thanks to the directors of the Veterans Administration Hospitals we undertook an experiment in intermittent hospitalization. We were successful in arranging for some of the patients to get out of the hospital and go back to their homes for a period of care and adjustment there. This required careful coordination between internists, nurses, physical therapists and occupational therapists, and psychologists and psychiatrists were brought into the program also. We carefully investigated everyone concerned and used the social service workers for appraisal of the home situation. With careful briefing of the patients and the friends and relatives at home, we were able to have the patients receive home care for periods of a month or more. They would then return to the hospital for further study, evaluation and briefing, with particular reference to rehabilitation and job finding. We organized a team involving internists, psychologists, and psychiatrists, together with occupational therapists and social workers, in order to make a full evaluation of each patient with respect to potential capacity for work. This was a direct effort to follow through with the program recommended by Doctor Howard Rusk and the Baruch Committee. We had considerable success in the effort and were able to place many of these patients in reasonably satisfactory jobs.

Certainly for these patients the ordinary chronic institution was no proper place. It divorced them from responsibility for family and children, and gave them no motivation on incentive for improving themselves and engaging in the rehabilitation program that might actually fit them for a job.

This plan is now being used in California, and also by the United Mine Workers. For the latter, we have organized programs which chronically ill patients can follow at home. These patients may be brought back to the hospital for intensive treatment for short periods at long intervals, together with evaluation studies and for briefing and motivation. This program of intermittent hospitalization and careful development of rehabilitation can certainly be made to work in a large number of patients who seem to be permanently disabled or crippled by chronic illness.

Harrower: You said psychologists and psychiatrists were "woven in," and I presume appropriately. Here, then, *is* a team that is working relatively well in helping to promote appropriate adjustments to make effective rehabilitation possible.

Brown: A great many of these patients can be handled wholly by the internists, but many others require group service and special service. Many need group survey in order to assure the best program. There are many such patients where deep psychotherapy is indicated, and there are many where special psychological evaluation may help. The psychologists do not have a great deal to do with the psychotherapy unless they are under the direction of the psychiatrist.

Holbrook: Doctor Brown has expressed his interest, his belief, and his faith that there are emotional and psychiatric problems in chronic illness, and that internists can profit from consultation with experts in these fields. Unfortunately, such experts are not always immediately available. Throughout the country generally, in the relatively small communities we need psychiatrists and clinical psychologists.

Rottersman: In specially developed clinic centers, whether in large or small cities, there may be reasonably available psychiatric and psychological aid for the internist dealing with chronically ill patients. On the other hand where such special facilities have not been developed, and this seems to be the case in most of the small communities throughout the country, patients whose neurotic illness is of such magnitude that it requires assistance from people skilled in psychological medicine, find that the internist taking care of them cannot get such help. There is broad need for an increased number and for distribution of psychiatrists and clinical psychologists to assist internists in adequate care of chronic illness.

However, in communities where highly skilled professional help in psychiatry and clinical psychology is not directly available, there may be enough aid from members of the family to help in satisfactory care of the chronically ill patient if the physician in charge is alert to the psychiatric and psychological needs of the patient. One may place any patient with chronic illness along a continuum ranging from those most severely disabled, grouped at one end, to those only slightly disabled, placed at the other end. In each and every patient along this continuum there is a multiplicity of factors at work. These factors collectively contribute to the maintenance of and to the degree of the disability. In each case, it may not be necessary to relieve the patient of each and every factor that militates against his enjoying a better state of health. In fact, one need not always relieve the patient of his most serious and obvious difficulties in order to achieve a modest but rewarding degree of therapeutic success. A few straws removed from the camel's back may improve the picture appreciably in a high percentage of cases.

For many chronically ill patients, the attitude of the physician—an iatrogenic factor—is often of importance in increasing and maintaining the extent of the disability. The adjustment of the chronically ill patient is often adversely affected by the attitudes and prejudices of his family, his friends, his relatives, and society as a whole. There is a pressing need for study and research in this area of professional and public education. Certainly, here, the psychiatrist and psychologist should and can, as a team, work together most effectively.

Lee: Let me speak from the standpoint of the low level of the general practitioner. One like myself makes thousands of home calls during the course of practice. Practical help is needed here, particularly when emotional factors develop, which are beyond the time or capacity of the ordinary clinician. I think I am rather better informed than most general practitioners about the help that can be given by psychiatrists and clinical psychologists. Most general practitioners are almost completely unaware that such help is available, let alone knowing how to use it. General practitioners would benefit

from learning what help can be given by psychiatrists and clinical psychologists, particularly in the emotional difficulties arising in chronic illness. Indeed publicity on this matter might alter the attitude of the general public. Consider what has been done with so many disease conditions. When I was a little boy, tuberculosis was a shameful thing. The attitude has completely disappeared. Broad publicity has brought about big changes in public attitude toward cancer, toward venereal disease, and the same sort of intelligent change in attitude could be developed with all sorts of chronic illnesses.

I think that the internist should have the primary responsibility of dealing with chronic disease. The clinical psychologist and psychiatrist can help by being available when the internist really needs their help in handling the complicated emotional problems that may arise. But the internist must be helped to realize when such aid will be of benefit to him, his associates, the chronically sick person, and the patient's family.

Better yet of course would be to develop clinical teams between internists, psychiatrists, clinical psychologists and others for the management of chronic illness in connection with group clinics. Here the pooling of group opinion and of group skills could certainly help in devising appropriate rehabilitation programs for individually chronically ill patients.

Garrett: Studies have shown that the goals that are set in rehabilitation programs for young people are less frequently achieved than the goals set for rehabilitation for older people with chronic illness. It may be that the difficulty here is not so much in the patient as it is in the members of the rehabilitation group. The goals of the chronically ill patient, as that patient may see them, may not coincide with the goals envisioned by the rehabilitation team. Here the clinical psychologist might aid as a consultant. The clinical psychologist might assist the rehabilitation staff members to recognize their own emotional investments and involvements resulting from their work with the patient. Such consultation might help the rehabilitation staff to understand what goals the rehabilitation group are setting for the patient, and how those goals may differ from what the patient hopes for himself.

Holbrook: It seems to me that we have been inclined to skirt the edges of the central problem before us. This problem concerns what can be done regarding the psychological factors which arise in chronic illness, and which may be deleterious to the welfare of the chronically ill patient. We all appear to be convinced that fear, anxiety, insecurity, and rejection may be part of the feelings a patient has who is suffering from chronic illness. Very little has been said with regard to what can be done about it. Our comments have been interesting, have been concise, but without commitment regarding possible solutions to the problem.

Some of us have felt that the method of caring for chronic illness as now exists is quite satisfactory. With this I do not agree. We have talked about teamwork between internists, psychiatrists and psychologists; we have discussed the pros and cons of leadership in such teamwork; we have discussed the application of such teamwork in special institutions; we have brought up the point of expanding the team concerned in caring for the chronically ill, and also the part that may be played by the general practitioner in this area.

Much study and many trials may be necessary before we can with certainty propose the ideal team relationship in caring for the chronically ill. The motivation of a patient toward rehabilitation, his psychological awareness with regard to his disability and his environment, the significance of prognosis in his attitude, must all be better understood by this physician. Failure to understand these aspects of the chronically ill may well be therapeutically disastrous.

We have much to learn regarding the variations in approach to the chronically ill patient from the standpoint of prognosis. Failure to take into account variations in psychological attitudes in chronic illness may result in emotional distractions time after time, interfering with satisfactory rehabilitation.

The personality of the members of the team who are caring for chronically ill patients is important. Many times chronically ill patients will fix their attention upon one member of the team. It might be the physical therapist who is keeping the joints moving. It might be the nurse who helps make the bed comfortable. It might be the clinical psychologist who seems to have the closest sympathy with the patient. I think the chronically ill patient will choose with whom he will be in rapport, and I don't think it matters which member of the team it is, if the attachment redounds to the benefit of the patient.

Certainly we must include consideration of the care of the chronically ill in our medical training program and in our post-graduate medical training program. Are we ready however to develop specific courses for such training? How far shall our training in medical schools include medical auxiliary personnel? Is formulated material available for instructional purposes? Are there adequate primers available dealing even with the most simple personality relationships that may be concerned in chronic illness? I wonder what role religion plays in the psychology of the chronically ill, and what its therapeutic possibilities are.

In focusing some of the matters before us in the form of direct questions of this sort, I am hoping that study groups in this and similar conferences may develop some suggestions and ideas that might fruitfully be applied to the direct practical problem of dealing best with the psychology of chronic illness.

Watson: I must admit that when I first heard the theme of this conference was to be "Teamwork with the Chronically Ill," I couldn't resist a "Yea, team!" As men of good will we are prepared to cooperate, but we must resist the temptation to cooperate merely for the sake of cooperating. We must examine the basis on which cooperation is not only desirable but also necessary. We must examine carefully what forms cooperation may take. I am reminded that historically we are going through something that was a source of discussion in child guidance clinics some years ago. This was epitomized in the deliberations on this topic at the meetings of the American Orthopsychiatric Association which, then and now, has as one of its requirements for membership, experience in a team relationship. In this specific instance the team consists of psychologist, psychiatrist and social worker. Their membership study of about three years ago is relevant to the present discussion. They found that teamwork, or as it is perhaps preferable to call it, coordinated

services, is not a simple unidimensional affair. Instead, several patterns of coordination emerged as being actually used in practice in orthopsychiatric work. I have selected three of these as being relevant to our interests in coordinated services with the chronically ill and will adapt them to our present interests. First, let me summarize on the blackboard:

PATTERNS OF TEAMWORK

Coordination by

1. All specialists available who carry out specific diagnostic tasks with the patient and pool information, and then carry out specific therapeutic tasks.
2. Consultant pattern.
3. Integration through therapeutic use of the most "suited" to the particular case after diagnostic phase.

Let us for the sake of simplicity consider only internists, psychiatrists, and psychologists as supplying the coordinated services with the chronically ill although obviously other workers such as physical therapists and nurses are involved.

The first pattern involves having all three specialists see the patient diagnostically each doing a specific task. There is then a staff conference with all present at which a plan of treatment is worked out. This is put into operation with each again performing specific tasks based upon differences in background and qualification. This may be called a fixed pattern in that whatever the specialists do, it is roughly the same in broad outline of function for each patient.

Perhaps the consultant pattern is the most common way of coordinating services. Here each specialist is available, but the specialist, whose patient it is, makes referral or does not make referral according to his best judgment. Some cases are seen alone, some by two, some by three. In Dr. Brown's description of his work given a little earlier we find an illustration of this pattern.

The third pattern is similar to the first in that certain traditional tasks are carried out by each specialist and a conference is held. In the therapeutic attack, however, I am reminded of Dr. Lhamon's earlier remark about some of the research on social relations at Harvard in which a task leader emerges. In this pattern the person to work therapeutically with the patient emerges from the staff discussion as the one judged most appropriate to work with the particular patient. In the present context this would sometimes be the internist, sometimes the psychiatrist, and sometimes the psychologist.

Shorr: Let me raise the question of the satisfactions that physicians and others may develop in teamwork. Speaking as an internist, I wonder whether or not we are prone to be satisfied with our ability to work out a routine for our chronically ill patients. Without a basis of comparison with the results that can be obtained with full cooperation of clinical psychologists and psychiatrists, maybe we are too smug. I think that many more internists than is the case now, who work with chronically ill patients, should try to see what can be accomplished by cooperation from clinical psychologists and psychiatrists.

Kuzell: Most of us seem to be talking about organizing a psychological fire department. We seem to be considering teamwork for taking care of chronic illness after it has occurred. How about medical and psychological teamwork in the prevention of chronic illness? It would seem that the clinical psychologist might aid greatly in preparing individuals who may suffer chronic illness to be better able to withstand the stresses of chronic illness. Maybe the clinical psychologist could help internists in the general effort at preventive medicine.

Holsopple: Our problem is not only that the internist doesn't know what the psychologist has to offer, but also that the psychologist doesn't know what the internist needs. Clinical psychologists could probably help greatly in Doctor Brown's intermittant hopitalization program by cutting off months of hospitalization through effective teamwork, but how can the psychologist really help unless he knows what is really desired, both by the clinical team and the patient?

Ham: Study and research in these problems might certainly help. Research in these matters, in a sociological sense, has hardly begun. The question of who should deal with chronically ill patients is usually determined by whom the patient goes to. A chronically ill patient usually seeks out an individual clinician for treatment. He may then be referred to an institution of some sort where lack of funds usually results in a creeping development of custodial care. Research programs might be set up for study between internists, psychiatrists, and clinical psychologists as to what problems are involved in the care of the chronically ill, so that conferences can be held with specific data dealing with the matter. All who are interested in the care of the chronically ill, including the chronically sick patients, need to learn from one another. There is a syzygy involved. This means that there are many connective and correlated matters in dealing with chronic illness that we would all be wise to realize.

Leake: I have some questions to raise. First, in attempting to develop medical and psychological teamwork in the care of the chronically ill, can we use the example of teamwork which has developed over many years in various child health programs, and which on another level entirely has arisen in connection with medical military services? Secondly, can we formulate the factors which are involved in effective teamwork between internists, psychiatrists, and clinical psychologists, in the care of the chronically ill? Third, can we obtain the interest, cooperation, and participation of the public in supporting medical and psychological teamwork in caring for the chronically ill? Fourth, would it be wise to devise methods of promoting the gradual evolution of medical and psychological teamwork in caring for the chronically ill; rather than trying to reach an agreement *a-priori?*

Fremont-Smith: A great many years ago at the Massachusetts General Hospital I began my training in psychiatry. I learned a great deal of my psychiatry from Sarah Evarts who was in charge of psychiatric social work and who, in some ways, had a better approach to psychiatric problems than some of my psychiatric friends. One of the problems that arose frequently in our discussions was the actual function of a psychiatric social worker. I would

say to her: "Sarah, what do you actually do—it seems to me as if you are doing psychotherapy and very good psychotherapy at that."

"No," she would reply. "We don't do psychotherapy; that is for the psychiatrists. We only treat the environment."

"But," I replied, "how do you decide which is the environment and which is the patient?"

"Oh," she said. "The patient is the one who goes to the doctor, and the environment is naturally the family.""

Then I said to her: "Sarah, I have a fantasy: on a certain morning there came to the Massachusetts General Hospital a mother holding a little girl of six by the hand. She explained to the admitting office that her daughter was troubled with bed wetting and she wanted something done about it. The child was referred to pediatrics, then to neurology, where X-rays revealed no spina bifida, and then at last she went to the department of child psychiatry where the psychiatrist saw the little girl as his patient. The psychiatric social worker soon found the mother sitting outside the psychiatrist's office and took her on as part of the environment! The mother revealed that she was very much troubled over her little girl's bed wetting, that it made her nervous and that she suffered intensely from headaches which were accentuated every time the little girl wet her bed.

In my fantasy I also saw on the same morning another woman coming to the out-patients department and at the admitting office she also explained that she was suffering from recurrent headaches. She was naturally sent to the medical department, then to neurology where the fundi and X-rays of the skull were negative, and then at last she went to psychiatry where the psychiatrist saw her as his patient. In the course of the discussions the woman revealed that she had a little girl of six who frequently wet her bed, and that somehow this made the mother feel nervous, and greatly accentuated her headaches. The psychiatric social worker was promptly dispatched to the home to see the little girl who, quite naturally, was part of the environment!

Watson: Since we have considered some of the factors concerned in teamwork, with particular reference to caring for the chronically ill, may we give some consideration to patterns of teamwork? It would seem that coordination between different workers toward the common goal can be obtained by (1) all members of the team being available and doing specific tasks; (2) by initial responsibility of one of the team members, supplemented by consultation where judged appropriate, and (3) integration of the team through the use of the most "suited" as therapist, after the diagnostic phases of the case have been covered by group agreement. Coordinated teamwork in caring for the chronically ill under the first instance would require *a-priori* agreement on individual responsibilities and specific tasks for each member of the team. The consulting pattern is probably the one followed in the usual pressure of medical service. Here the success depends upon the sense of responsibility in the internist in seeking psychological aid when emotional problems arise. Development of the team by gradual agreement on the most "suited" as leader may work out to be the most satisfactory procedure. The most "suited" may vary with different groups. In some instance it might be the physio-

therapist while in others the psychiatrist or occupational therapist. Sometime it might be the nurse. And of course it could be the internist or the clinical psychologist.

CHAPTER VIII

COMMUNICATION IN TEAMWORK FOR THE CHRONICALLY ILL

COMMUNICATION BETWEEN PSYCHOLOGISTS AND INTERNISTS

MOLLY HARROWER

As was pointed out before by Dr. Fremont-Smith, in order to communicate *with* some one, and not make statements *at* him, we have to be assured that his "receiving set" is tuned in appropriately. Distrustful attitudes, anxiety, anger, prejudice, feelings of insecurity, may all be ways in which the human receiving set can be detrimentally influenced. Simply restating a message, increasing the power of the transmitter, is going to get us nowhere if no one is tuned in to listen.

But there is something more to communication than this. For a message may not be understood, despite adequate transmission and an adequate set to receive if, for instance, that message is sent in a foreign language or, if no one at the receiving set end thinks it is worth while tuning in the set for a particular transmitter.

In speaking about the communication between the general physician and the psychologist in this way I am doing more than just expanding Dr. Fremont-Smith's nice analogy. For in the problem of communication between the physician and the psychologist we have an example, par excellence, of the two above mentioned factors, namely, a lack of a common language in which to communicate, and a lack of awareness on the part of the majority of physicians that it is worth "tuning in" on the set when a psychologist speaks. There is further a lack of awareness on the part of the psychologist that he actually has information which may be of value to the general physician.

One of the major aims of this Conference, and of this publication, has been to bring to the notice of those who deal with chronically ill persons the actual and potential contribution of the clinical psychologist as a member of the medical team. While

there is probably no one, professional or otherwise, who does not know what a doctor does, it is safe to bet that the functions of the clinical psychologist are only hazily envisaged, except by those in the medical profession who happen to work closely with them. Since the profession is just in its infancy, when compared with that of our medical colleagues, this does not exonerate clinical psychologists from explicitly stating those skills and abilities they possess which bear on the age-old problem of human suffering, mental and physical.

What is the clinical psychologist's function? What is he equipped to do professionally, and how can he contribute to the care of the chronically ill? It would be generally agreed, I think, that the clinical psychologist's role is threefold: personality appraisal, research, and some form of counseling or guidance.

The clinical psychologist is equipped, in the first place, with special techniques for making detailed psychological examinations, such that he can appraise, assess or evaluate an individual's intellectual qualities, his emotional equipment, his liabilities, his assets, his creative potentialities, his psychological deficiencies. Sometimes the pattern of these assets and liabilities is "typical" of certain kinds of neurotic difficulties, so that a psychiatric diagnosis within the classical framework can be given. But the extent of the information available from a careful and detailed psychological examination goes considerably beyond a mere diagnostic label.

It would also be generally accepted that the psychologist is trained in research methods and techniques. Outside his strictly clinical training, an important inheritance from his background as an experimental psychologist, he is equipped to design and execute controlled, experimental procedures, and to produce statistically reliable results.

As a third part of his training and equipment there are his skills, in counseling or guidance, skills which inevitably approach and shade over into the area of psychotherapy. A small group of psychologists are fully trained psychoanalysts. Many have had personal analyses and systematic training in therapeutic techniques.

It is this guidance area of the psychologist's function which at the present time had led to controversy, in some instances,

between members of the psychological and medical profession. The Conference as a whole did not discuss or evaluate this controversy in any way. It should be mentioned here only because, in view of the enormous psychological potentials for psychotherapeutic work within a medical setting, the small area of specific controversy, namely, independent private practice of psychologists in psychotherapy, is relatively unimportant.

Let us consider each of the aspects of the psychologist's job with reference to the specific problems of interaction with the general physician, and collaboration in the care of the chronically ill.

Although diagnostic evaluations have, in the main, been requested by psychiatrists, and have become part of the professional inter-action between clinical psychologists and the psychiatrists, there is every reason to believe that personality appraisals may be equally helpful in organic disturbances. It is at this point that it becomes imperative that the physician have some idea of the kind of information which the psychologist can give him and, conversely, that the psychologist have some idea of the actual problems that are confronting the physician in any given case.

The question may be, for instance, as to whether or not psychological factors could contribute to an actual physical diagnosis, as in the case of multiple sclerosis where the initial picture is often a confusing one. The two charts which are now given illustrate psychological material directly relating to the personalities of two patients in both of whom multiple sclerosis was suspected, but in only one of whom was the diagnosis confirmed.

It will be seen that the psychological pictures of patients *A* and *B* differ quite markedly and in these instances, even without knowing anything of the detailed working of any of the psychological tests, it is possible from this summary for the physician to get an overall picture of two very different personalities. From his psychological background the psychologist knows that Patient *A* may be diagnosed as psychotic and, in the light of detailed studies on patients with multiple sclerosis, Patient *B* falls into and approximates a composite picture obtained from two hundred patients with this disease.

Such a chart as this is an attempt at a minimum common language, a framework out of which the physician and psychologist can both operate in order that the psychologist's report may be

Summary of Test Findings—Patient A

Manner During Test				
Overtly Distressed	Uneasy	Relaxed, Interested	Competitive and Tense	**Hostile**
Rate of Performance				
Very Slow	Slow	Average	Speedy	Excessively speedy
I.Q. (Bellevue-Wechsler)				
Below Average	Average	High average	Superior	Very superior
Productivity (Rorschach)				
Meagre	Average	Rich and well ordered	Rich, but chaotic	Chaotic
Relation to Reality (Rorschach, Bellevue Wechsler, Drawings)				
Fanatical exactitude	Not noticeably disturbed	Firm and good	"Artistic leeway"	**Loose**
Usual—Unusual Thought Content (Rorschach, Unpleasant Concept)				
Stereotyped	Average	Original	Bizarre qualities offset by other features	**Bizarré**
Emotional Tone (Rorschach, Szondi)				
Lacking, repressed	Struggling for expression	Warm, readily available	Getting out of hand	Emotions rampant
Constructive Fantasy (Rorschach)				
Absent	Average	Active	Active +	Topheavy, withdrawal
Passivity—Aggression (Rorschach, Szondi. Unpleasant Concept)				
Hampering passivity	Insufficient drive	Sufficient drive	**Aggression +**	Overpowering aggression

more meaningful and convincing, and in order that the physician may compare, with ease, any two patients or one and the same patient after a lapse of time.

Summary of Test Findings—Patient B

Manner During Test

Overtly distressed	Uneasy	**Relaxed, interested**	Competitive and tense	Hostile

Rate of Performance

Very Slow	**Slow**	Average	Speedy	Excessively speedy

I.Q. (Bellevue-Wechsler)

Below average	**Average**	High average	Superior	Very superior

Productivity (Rorschach)

Meagre	**Average**	Rich and well ordered	Rich, but chaotic	Chaotic

Relation to Reality (Rorschach, Bellevue Wechsler, Drawings)

Fanatical exactitude	**Not noticeably disturbed**	Firm and good	"Artistic leeway"	Loose

Usual—Unusual Thought Content (Rorschach, Unpleasant Concept)

Stereotyped	**Average**	Original	Bizarre qualities offset by other features	Bizarre

Emotional Tone (Rorschach, Szondi)

Lacking, repressed	Struggling for expression	**Warm, readily available**	Getting out of hand	Emotions rampant

Constructive Fantasy (Rorschach)

Absent	**Average**	Active	Active +	Topheavy, withdrawal

Passivity—Aggression (Rorschach, Szondi, Unpleasant Concept)

Hampering passivity	**Insufficient drive**	Sufficient drive	Aggression +	Overpowering aggression

While the use of diagnostic evaluation for patients with organic disturbances, acute and chronic, does occur in some cases of collaboration between psychologist and physician, the possibilities of the psychologist, in his research capacity, have not really begun to be utilized within the framework of chronic disease. An excellent resume of psychological work with the handicapped exists in the book, "Adjustment to Physical Handicap and Illness: A Survey of the Social Psychology of Physique and Disability" by Roger Barker and his collaborators. But still a vast, unexplored field awaits a collaborative venture. Some of the problems in this research framework have been touched on in the report of the subcommittee dealing with this topic. Here one might mention such problems as a study of patients approaching death, as was suggested by Dr. Lawrence Kohn, detailed studies of patients in various phases of convalescence, those making fast as opposed to those making slow recovery from successful surgical procedures. Studies of patients during various phases of chronic disease as, for example, those which have been attempted in multiple sclerosis in a comparison of early cases with those in periods of remission. As more and more individuals are studied, through group testing procedures in schools and colleges, it will become possible ultimately to correlate initial personality structure with diseases which occur later in life. The longitudinal section approach, long-term studies, are few and far between but are vitally necessary if we are ever to link psychological characteristics with terminal diseases.

The psychologist, in his therapeutic capacity, brings to the field a new interest in the therapeutic process. His training has included studies of learning, habit formation, and often group dynamics. The whole question of the utilization of the projective techniques in a direct therapeutic setting is just emerging as a new technique. The evaluation, psychologically, of patients who have been treated therapeutically by different methods would also fall into the psychologist's province. For example, what would a group of arthritic patients look like in the psychological tests who had undergone not only all available physical therapy but had, let us say, over a period of two years participated in group therapy sessions? Psychological studies at the beginning and end of such a two-year period might answer the question to

what extent the psychotherapeutic process could be said to have changed the personality structure and/or the physical symptoms.

How is it possible that better communication and better utilization of the psychologist's services can be brought about? In the first place, one would hope that sooner or later lectures would be introduced into the medical curricula describing the place of clinical psychology in psychiatry and general medicine. Secondly, there should be an attempt to publish in medical journals collaborative reports and research, in general as well as technical terms, so that a wider audience within the field of general medicine could get a nodding acquaintance with such studies. Thirdly, in the training of psychologists there should be included actual experience in general hospitals, where the student would be exposed to the problems of sick persons, and begin early to feel at home in the hospital setting.

COMMUNICATION BETWEEN PSYCHIATRIST AND PSYCHOLOGIST

EUGENE C. MCDANALD

As a psychiatrist I am not in position to define the role of a clinical psychologist with clear-cut certainty. In general, it is my impression that he is concerned about human beings, especially their interpersonal relationships and their way of life; more especially he is concerned with their psychological adjustment problems, that is, with the determination and evaluation of specific capacities, abilities and characteristics relating to personality adjustment and with the study and application of techniques for improving adjustment. Within this broad definition is found many variations in the actual services performed by the clinical psychologist; yet essentially they consist of one or all of the following:

1. The administration, scoring and interpreting of psychological tests for the purpose of assessing an individual's personality structure and integrity, that is, elucidating the dynamics of his behavior with accent on his primary interests and motiva-

tions, his intellectual potential and efficiency, and his special abilities and disabilities. In reporting on these tests the psychologist does more than report the findings of the examination. He, in effect, makes a clinical appraisal such that the referring physician may come to a better understanding of the problems presented by the patient.

2. Outlining recommendation for special training, vocational and educational rehabilitation on the basis of psychological tests and other information.

3. Conducting personal counseling and psychotherapy in consultation with a medically trained psychotherapist.

4. Research in the broad areas of personality theory, measurement, growth, and change.

Within the specific area of personality assessment, which is of special interest to the psychiatrist, an important activity of the clinical psychologist is to contribute to a greater understanding of the patient through the interpretation and evaluation of the findings derived through the use of standardized psychometric techniques. To assist the psychiatrist (as well as any other medical person) interested in understanding the sum total of the patient's personality functioning, the psychologist, on the basis of the test formulations, can:

1. Aid in differentiating between a neurosis, psychosis and organic disorder.

2. Evaluate the intellectual functions with respect to potential, efficiency and special abilities.

3. Define the personality structure in terms of its chief characteristics and temperament.

4. Note the presence and severity of anxiety as well as the defensive mechanisms, since frequently the root of the patient's conflict can be determined by psychological testing.

5. Uncover ideational content and fantasy life.

6. Determine the degree of impairment of psychological functions.

7. Measure the presence or absence of improvement as a result of therapy.

8. Point up the personality resources and prognosis.

9. Indicate the reactions to specific stress or trauma, such as illness, injury, or accident.

In communicating his findings to those requesting his services the psychologist most often utilizes a written report and it is in this area that the psychologist and the referring psychiatrist may find themselves unable to understand each other. While there should be only minimal disagreement between the psychologist's interpretation of test behavior and the psychiatrist's interpretation of so-called clinical data, there frequently results a lack of consensus in their respective evaluations. This can only be due to a lack of communication (given competence on the part of the psychologist and the physician) since both are studying different samples of behavior, the essence of which is in the dynamics they *have in common*, and as such should be amenable to tranlation into a common language. For communication to be effective from the psychologist's standpoint, he must consider several factors: (1) What is it that the referring physician wishes to know? (2) What is the theoretical orientation and diagnostic bias of the referring physician? In other words, what "language" does he speak?

In connection with the first item it is an absolute necessity for the sake of economy of the psychologist's efforts that the physician indicate to the psychologist what it is that he wishes to know about his patient. There are scores of psychological tests, many of which are not only very time consuming, but not needed for getting the information requested. If the question, for example, is simply one of determining the intellectual level of an individual, such information from the physician will narrow considerably the number of tests to be administered. Or if diagnostic classification is the question, then an extended study of fantasy and defensive mechanisms is not in order. In short, the psychologist tries to meet the psychiatrist's request by using selected tests that will yield the information desired.

Relative to the second item, the psychologist, to be most effective, must be familiar with the theoretical orientation and diagnostic bias of the referring physician. Many blocks in communication are due to the different nosological and theoretical learning of the would-be collaborating psychologist and psychiatrist, and in the use, by the psychologist of highly technical language. Two psychologists may communicate with each other in highly abstract symbols, but if the psychologist is to communicate with those to whom the report is going he must use

language comprehensible to them. This necessitates the use of language within the frame of reference of the referring physician. Otherwise, a technically perfect and valid report remains worthless. Such technical language may be peculiar to the field of psychology or to a particular theory of personality, but whatever the source, unless it is meaningful to the reader and conveys what the author wishes, the psychologist might just as well have saved himself the effort of his labors. Assuming, however, that the psychologist and psychiatrist are professionally or technically in rapport, for communication to be most effective more than a written report is needed. The report should ideally be talked over since clarification, the essence of communication, can usually be brought to bear on one or more areas.

The penetrating "quick peek" into personality structure provided by psychological testing is as reliable as the tester and is communicable in proportion to the mutuality of the orientation and clinical maturity of the psychologist and psychiatrist.

COMMUNICATION WITH THE CHRONICALLY ILL PATIENT

CHAUNCEY D. LEAKE

In briefing first-year medical students, we often suggest that it will be good for them to get some sickness or injury while in medical school, mild enough for safety but serious enough for hospitalization with clinic follow-up. Under these circumstances they might learn at first hand how a patient appreciates the psychological factors which are part of satisfactory medical care. It might dawn on them, as often happens, that the member of the medical group whom they like most, and who they think really does most for them, is not the awesome physician, not the impersonal resident, nor the harried interne, not the starched nurse, nor the anxious aide, but the quiet maid who brings the food tray, makes the bed and chats for a moment. If this is the case, as occurs too often, it is largely because the maid has succeeded, however, unconsciously, in communicating with the patient she serves, where the trained professional personnel have failed. The maid, in her way, has been able to make her patients realize

that she is doing her job for them, and is kindly and sympathetically interested in them. Only too frequently do professionally trained personnel consider their patients as "cases." When doctors and nurses take an impersonal attitude toward their patients in thinking of them as "cases," there is scarcely any basis for an interpersonal relationship that may permit satisfactory communication.

Communication with a chronically ill patient, to be successful, requires full appreciation on the part of the professional personnel involved of what the emotional and psychological situation may be *in that particular patient.*

We have all been intrigued to have Frank Fremont-Smith tell us that receiving sets vary greatly in their ability to pick up transmitted energy. A few patients may have simple auditory or visual difficulties, others have language barriers, some have conceptual ignorance or low intelligence, many have divergent value referants, some have emotional resistances conditioned by various associations, and practically all are frustrated by the problem of readjusting their idealized picture of themselves as healthy normal human beings to the devalued depressing version of themselves as chronic part-selves, with emasculated egos. Under these varying conditions the professional medical attendants need training in recognizing the variations in the receiving sets as well as in modifying the character of transmission, if communication with the patient is to proceed. Further, the professional personnel need training in prompt adjustment of their own receiving sets in order to get fully what such patients may be trying to transmit.

Many semantic problems are involved in communication with the chronically ill patient. These cover a wide range of factors varying with each particular patient. The meanings of common symbols may differ with different people dependent upon social status, working associations, and recreational activities. Of paramount importance is the basic belief of an individual. This is commonly designated as an individual's "religion." What does an individual really believe with regard to himself and his environment? Whatever an individual's belief may be, communicating signals coming into such an individual are filtered through

the meshes of that person's belief. Meanings are interpreted subjectively. Under these circumstances there may be superficial appearance of agreement and understanding with regard to communication, with however a fundamental difference in attitude, motivation, understanding, and appreciation between the communicants.

These factors suggest the importance of an intensive and intelligent effort on the part of physicians, nurses, psychiatrists, psychologists, social workers, and others who may be concerned with the care of the chronically ill in learning the peculiar emotional make-up and "credo" of each individual chronically ill patient with whom they may be concerned. Such knowledge may aid greatly in effective communication. More importantly it may aid in helping the chronically ill patient to understand that those who are trying to take care of him are really interested enough in his situation to learn as much about his personality as possible.

In appraising the personality of chronically ill patients, clinical psychologists have semi-quantitative tests available, the results of which may be of great help to all members of the medical team who are taking care of such patients. The careful consideration of the results of such personality appraisals in connection with chronically ill patients may help to make communication with such patients easier than would otherwise be the case. Certainly, in order for chronically ill patients to be able to take advantage of the great help that may be given them in rehabilitation, it is necessary to make every effort to let them know what can be done for them, how they may cooperate in the doing, and what the possible results of the rehabilitative effort may be. Success in such communicative efforts may have much to do with the motivation of the chronically ill patient.

The consideration of semantic principles may aid greatly in the effective functioning of teamwork on behalf of chronically ill patients. Fortunately, a well disciplined development of semantics has occurred, so that all members of the team caring for the chronically ill patient may become familiar with and try to apply such principles of communication as may be pertinent to the care of the chronically ill.

THE ROLE OF THE COMMUNICATIVE AND GENERAL SEMANTIC DISCIPLINES IN COORDINATIVE EFFORTS TO FASHION A MEDICAL AND PSYCHOLOGICAL PROGRAM FOR THE CHRONICALLY ILL

Russell Meyers

It has by now become apparent to many contemplative students that science in general, "pure" and "applied," and medicine, psychiatry and human psychology in particular do not exist *in vacuo* but constitute integral parts of a highly complicated cultural milieu—familiarly known to us as "Western Civilization." As such, they are inevitably involved in the processes of emergent evolution of the culture-at-large, influencing it and being in turn influenced in a never-ceasing dynamic flux of energy exchanges. If, as Heraclitus asserted 600 years B.C., a man can never step twice into the same river, we may remark with equal confidence that a man can never step twice into the same social milieu, much less into the same "science," "medicine," "psychiatry" and "psychology."

One of the recently acquired characteristics of our civilization is that of being a culture-studying culture. Historically speaking, this is unique. The exercise of systematically scrutinizing the unvoiced (and ordinarily unidentified) axioms that underly the codes by which we live appears to be without counterpart in past records of mankind. Its impact thus far has been felt only by the relatively few sensitive workers on the periphery of scientific inquiry. Even among these, its potential impact upon the foreseeable future must remain for the present largely speculative. But that such potential is recognized at all and that, in addition, a measure of faith prevails among the informed few that through competent control such exercise may ultimately come to engender a salutary influence upon the lives of the many are circumstances eloquently expressed in the growing number of conferences of the sort in which we are here engaged.

What we enter upon in this conference, what blueprints we sketch out, however tentatively, must inevitably affect all that comes after us. The reason for this is that the "objects" of our

study—men, women, children, groups, their physio-psycho-sociologic patterns of action, reaction and interaction and the cultural concepts identifiable among them—are perceptibly changed by the very process of their being examined and evaluated. There is, in a word, an unavoidable *self-reflexiveness* involved here which has only the most tenuous counterpart, if any at all, in the physical sciences like astronomy, meteorology and physics. In view of this it would appear that as we enter upon sober deliberations, the records of which may for some years be consulted by our colleagues and professional descendants, we should keep ourselves continuously aware of the gravity of our responsibility to "make sense" in our deliberations.

In no small measure our endeavors consist in an exercise in communication—intrapersonal as well as interpersonal. At the outset we have every reason to suppose that our constituents come together in good will and with sincere intentions to commune for worthy ends. Unfortunately, as communication engineers are well aware, while these are necessary prerequisites to joint achievement, they are not in themselves enough, for the binary units of information exchanged among us and upon which any palpable recommendations must be structured can be "real" *or* "spurious," *i.e.*, can lead to sound predictions, progressive adaptation to and control of the intra- and extra-organic environments *or* to the frustrating disappointment brought by defective predictions, maladaptation compounded *ad nauseam* and chaos.

Medicine has for long embraced certain *a priori* ideals: (1) that of preserving the patient's life as long as possible; (2) that of providing optimal function for the already damaged patient; and (3) that of offering comfort and assurance to the incurable who stand in sorrow and in pain. Towards these broad and laudable aims prophylactic and therapeutic measures for the control of "disease" have been gradually and painstakingly evolved. Lately, becoming aware of the nebulous character of the distinctions between "disease" and "health," we have come to envision an extension of the programs of medicine, psychiatry and human psychology, namely, to go beyond the treatment and prevention of "disease" and embrace the population at large, the goal being nothing short of helping all who desire to *maximal*

efficiency and happiness within the framework of their own capacities and the assets and liabilities of their socio-cultural milieu.

All these are broad goals and of them, as ideals, little can be gainsaid. Our difficulties begin when we attempt (1) to translate them from the general to the particular, such as we are now attempting in connection with the chronically ill, and (2) to determine the mechanisms and pathways by which the adopted particular goals may be reached. To these ends it has seemed promising to pool the efforts of a number of experts in the medical and paramedical areas. We envision that the benefits of "interdisciplinary integration," "cross-fertilization of ideas" and the like may somehow sprout from such efforts and that some of these sprouts may even bear fruit.

Few of us are so naive as to believe that either the particular goals we seek or the means for achieving them are at present well defined. If this conference should succeed in defining them and in designing the means of their implementation it will exceed the fondest hopes of all who participate in it and earn the gratitude of numerous professional colleagues throughout the world. But if past experience is any guide, the most sanguine among us must realize that we shall have to do a great deal of groping, often without a clear awareness of just what we are after, before clear concepts and flashes of insight begin to emerge. Such a *modus operandi*, while unavoidable, is bound to leave some with misgivings.

Meantime, what have we to begin with? What tools are at hand to apply to the relatively amorphous materials on our work table? What, if anything, has in the past served us most consistently in the endeavor to define and solve the problems to which we have addressed ourselves?

To the writer it appears that we can count implicitly and explicitly on three agents: (1) the ethical orientations inhering within operational philosophy; (2) scientific method; and (3) the metalinguistic orientations provided by general semantics. These merge insensibly into one another and, except for purposes of classroom identification, ought not to be thought of as compartmentalized. They may be briefly epitomized as follows:

The Ethics of the Operational Philosophy of Science
(A. Rapoport)

1. A preference for truth (desirably, "predictable").
2. Ready accessibility of available knowledge.
3. Freedom to pursue any desired inquiry.
4. Non-authoritarianism.
5. Amenability to change.

Scientific Method
(K. Pearson)

1. Observe and record phenomena.
2. Arrange, order and classify the facts.
3. Derive hypotheses, theories, principles, laws.
4. Observe and record more phenomena.
5. Recheck hypotheses, etc., vs. 4.
6. Retain, revise or reject the hypotheses as dictated by 5.
7. Repeat 4, 5, 6 indefinitely.

Concerning these agents little elaboration is required in view of the background of the present group. As to the third, Metalinguistic Orientations Provided by General Semantics, a few comments may help illuminate the issues. First, as to the term "metalinguistic." This neologism simply refers to a language for talking about language—an obviously essential tool in any culture-studying culture, since the language of one's culture appears to be not only a potent determinant and sustainer of folkways, mores, health totems and taboos, but a serious limiting factor, often exasperatingly circular in its effects, in efforts to study and evaluate the linguistically-bound culture itself. General semantics may justly claim to have contributed to the development of the now available metalinguistic tool. For example, with regard to the exercise of scientific method, it urges the necessity at the outset of any inquiry of asking a *meaningful* question. This springs from its conviction that the terms of an answer are already inherent in the terms of the question and that vague and nonsensical questions cannot fail but yield vague answers. Its interest in the "meaning of meaning" extends far beyond the fatuous definition of one term by another which characterizes so many dictionary definitions and so much of medical and psychiatric jargon. It prefers to define a word in terms of its non-verbal

referent, accepting the word as a sort of promissory note which can and will be promptly "cashed" for goods on demand. It does not, as many commentators have erroneously supposed, summarily reject terms which lack such referents. On the contrary, it acknowledges their high usefulness to the progressive development of science and society and advocates their employment, but insists that their man-made character be recognized and, above all, that they should not be given priority or inviolable status over terms with empirically demonstrable referents.

Consistent with all this, general semanticists recognize that, since every "fact" requires an observer-recorder and that the observer-recorder is inescapably part of the "fact" he reports, the differences between facts and hypotheses are more apparent than real. Going beyond this, they are impressed that the scientific act of arranging and classifying "facts" is a biologic process determined and limited by the classifier's own neuromuscular endowment and that classifications often refer much more to (and therefore tell much more about) the classifier than to the "natural" phenomena which are naively presumed to lie outside the classifier's skin and to be invariant to him.

These orientations become of vital and practical importance in every act of communication, in every question asked, in every proposition stated, in every solution reached. Studies in group dynamics have repeatedly demonstrated that failure to exercise them is commonly responsible for the bogging down of human discourse.

The general semanticist is likewise impressed that we can never know "all" about, much less tell "all" about anything; and that "knowing," in a rigorous epistemologic sense, is determinable only by *what the knower does*. These principles accepted, he remains aware that merely because two individuals engaged in communication use the same words, they do not necessarily have the same perceptual responses, hence that they are in constant danger of unwittingly getting out of communication with one another. He therefore *expects* misunderstanding and counts himself a skillful communicant to precisely the extent to which (a) he proves operationally capable of anticipating the *ways* in which, as speaker, he may be misunderstood and, as listener, may misunderstand and (b) he is able to devise and

improvise measures to *reduce* misunderstanding to a minimum. Like the communication engineer, he deals with communication in terms of probability theory.

Finally, the general semanticist insists on a freedom to challenge the occult assumptions and circumscribed logical systems detectable behind the terms of many questions and their answers. He is careful not to cast reflections on the personal or intellectual integrity of those who ask the questions or utter replies, for he remains always cognizant of the fallibility of the best of us. But he does not expect to lend his energies, time and money to support research on questions such as, "Do round squares eat red cabbage?," "Who are the ten sexiest men in the world?," "Are American women moral?" and "What is the role of the extrapyramidal system as the center of consciousness and libidinous drive?" In brief, he is aware that something more than the observance of prescribed grammatical rules is involved in raising questions worthy of his address.

Where the aristotelian logic of identity and classes applies, the general semanticist is prepared to exploit it. But he is impressed that there are other useful ways of talking about relationships between the phenomena of this world than in the aristotelian terms which pervade most erudite discussions; and that the rules for manipulating such other methods of expression must if discussion is to be profitable, differ from those laid down by aristotelian logic.

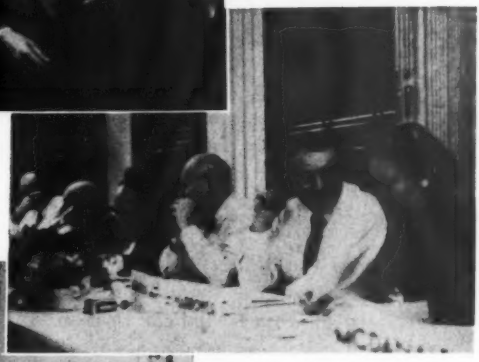
This is not the place to synopsize *in extenso* the role that general semantic formulations might play in the present conference. However, to invite interest in its possibilities, the speaker has drawn up a list of bed-rock terms, our linguistic *damnosa haereditas*, which make their appearance in every serious discussion and which, from the semantic viewpoint, are particularly likely to prove the crucial points beyond which profitable communication begins to break down. These are the terms and dichotomies referred to:

1. Function.
2. System.
3. Cerebral Centers.
4. Structure and Function.
5. Heredity and Environment.
6. Objective and Subjective Data.

7. Reality and Unreality (real, really, realness, etc.).
8. Abstract and Concrete.
9. Male and Female.
10. Mental and Physical (Mind and Body, Psychologic and Physiologic).
11. Cerebrospinal and Autonomic Nervous Systems.
12. Sensory and Motor "Functions" of the Brain.
13. Pyramidal and Extrapyramidal "Motor Systems."
14. Cortical and Thalamic "Levels of Function."
15. Intellect and Emotion.
16. Conscious and Unconscious (consciously, consciousness, etc.).
17. Voluntary and Involuntary Behavior (Will and Reflex).
18. Thought and Speech.
19. Painful and Pleasurable States.
20. Health and Disease.
21. Organic and Functional Diseases.
22. Neurosis and Psychosis.
23. Idiopathic and Symptomatic Diseases.

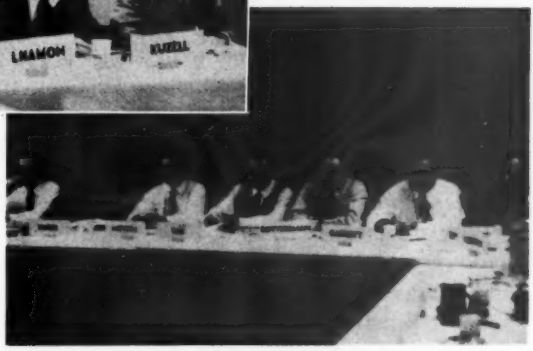
If, in this conference, we can somehow avoid the pitfalls covered by these terms, we shall have come close to making history. We may even succeed in sketching a part of the large blueprint we hope eventually to design.

A light moment
Fremont-Smith,
Harrower, Kubie,
Kohn



Conference Chairmen
also confer: Jacobsen,
Shorr, Seidenfeld,
Fremont-Smith,
Holbrook, Harrower

A serious moment



Attentive
consideration

Candid Conference Snaps mostly by Lawrence S. Kubie

CONCLUDING NOTE

CHAUNCEY D. LEAKE

The proceedings of a conference such as this give little idea of the personal aspects of living and working together that can make an intellectual meeting enjoyable, stimulating, and productive. In this instance, the conferees lived in pleasant rooms at the Jack Tar Courts on the beach, and within walking distance of the Medical Branch buildings. After informal breakfasting together, the group would stroll in conversation to the meeting rooms. Sessions were held in a special conference room on the main floor of the William Keiller Laboratory Building. Tables were arranged in an open horse-shoe, around which the conferees sat. Coffee breaks in the middle of the morning and afternoon were held in the Student Lounge across the street in the Ashbel Smith Building. Luncheons were held in the John Sealy Hospital. and special dinners were arranged at the Galveston Club.

At one of the dinners, Doctor Truman G. Blocker, Jr., Professor of Plastic Surgery and Dean of the Medical Faculty at The University of Texas Medical Branch, gave an illustrated discussion on psychological factors in the care of acutely burned patients. This was followed by a helpful general discussion in which it was agreed that modern psychology could aid greatly in all phases of medical practice, including acute situations as as well as chronic illness.

A feature of one of the evenings, following dinner, was a public discussion by Doctor Lawrence S. Kubie on a coordinated training program for a new professional group combining psychology, medicine, and psychiatry. This appears in Chapter V under the title "The Pros and Cons of a New Medical Discipline."

Busy sessions left little time for recreation, yet some of the conferees went swimming daily, and all seemed to enjoy the leisurely opportunity for informal discussion in addition to conference activity.

Considerable correspondence was involved between those who organized the effort and those who were invited to participate. Through this correspondence the general program of the conference was developed. In one letter to the conferees, January 7,

1954, it was indicated that some of the general subjects likely to be discussed would include:

1. Size and character of the problem involved in the care of the chronically ill.
2. Relation of psychological factors to rehabilitation.
3. The relation of medical and psychological teamwork in the care of the chronically ill to activities of national and state agencies and to the effort of private agencies.
4. Factors in the development of teamwork between internists, psychiatrists, and psychologists in the care of the chronically ill.
5. Methodology to be developed in the proposed teamwork.
6. The implication of the effort in medical, nursing, and health personnel training.
7. Aspects of the problem involving the patient, the patient's family, and those who are concerned in handling the patient.
8. What particular contributions can be made by trained clinical psychologists in the proposed teamwork?
9. Special psychological factors involved in handling chronic illness in children and the aged.
10. Educational efforts directed toward developing and maintaining good morale in the chronically ill and among those who take care of them.

In the first letter of invitation to attend the conference it was stated that "the purpose of the conference is to bring together for informal discussion and exchange of ideas a representative group of physicians, psychologists, and psychiatrists. . . . This conference has been suggested by the expressed need of internists in obtaining aid from psychiatrists and psychologists in the care of the chronically ill. Current recognition of the many urgent psychological problems facing the chronically ill and their families, for which internists are not fully prepared, and for which psychiatric personnel is not available, indicates teamwork between internists, psychiatrists, and psychologists is the best way of meeting a rapidly developing serious situation. It is hoped that an informal interchange of ideas between representative leaders in these fields may bring proposals for effective cooperation among all concerned in handling chronic disability in a more satisfactory manner, than is usually the case."

The contributions at the conference and the material gathered here for publication, indicate clearly that the prime purpose of the conference was well met. Without being requested to prepare specific discussions, the conferees offered their individual comments in such a way that they could be classified in such specific areas of interest as: (1) The care and treatment of the chronically ill; (2) psychological problems of the chronically ill; (3) problems of teamwork in the care of the chronically ill; (4) problems in teaching and training medical and psychological personnel for the care of the chronically ill; (5) problems of interdisciplinary research in the care of the chronically ill, and (6) communication in teamwork for the chronically ill. The synthesis of the various points of view into one general program was attempted by general discussion.

Specific recommendations were considered by three study groups, which were organized on the second day of the conference. One of these study groups proposed more effective rehabilitation efforts in every community with psychological aid. It also suggested that facilities for the care of the chronically ill should be centrally located in order that the chronically ill patients and those who take care of them need not feel rejected by being in an institution far out in the country where all may readily and conveniently be forgotten. This group also urged that rehabilitation councils be established in communities in order to devise local programs for satisfactory care of the chronically ill.

Another group considered programs for teaching and training the personnel needed in a medical-psychological team for care of the chronically ill. The complexity of research in regard to the proposed teamwork was well explored by a third study group. In all instances there seems to have been full appreciation of the many psychological and unconscious neurotic factors that might provide resistances and barriers to effective teamwork or to satisfactory communication between professional personnel, and the chronically ill patient. On the other hand various suggestions were made for promoting more healthful and wholesome adaptations to the conditions of chronic illness, with various proposals for effective methods of psychological support for chronically ill patients.

In addition to the rather obvious subjects for specific consideration, there was also discussion of the influence of natural surroundings in treating chronic illness, a significant commentary on devaluation in chronic illness, a similar broad consideration of chronic loneliness and anxiety in chronic illness, and a discussion on the inevitable period of mourning in the early stages of chronic disease. An attempt was made to devise methods of preventing focusing of frustration in chronic illness. A wholesome point of view was offered in regard to the broad training of the general practitioner to act effectively as a one-man team, capable of handling the psychological difficulties as well as the medical problem of chronically ill patients. There was technical discussion of differentiation and integration in teamwork, and there was considerable healthy criticism of the general concept of team activity. Emphasis was placed on the modern medical curriculum as aiming toward total care for the sick people. Much specific detail was offered in a training program for medical and psychological personnel in regard to preparation for rehabilitation efforts with the chronically ill.

The scientific challenge in chronic illness was explored both from the standpoint of psychology and from the basis of definitive medical care. Much emphasis was placed upon the part to be played by the chronically ill patient and groups of such patients in the study and management of chronic illness. Finally there was significant discussion on various aspects of communication between psychologists, internists, and psychiatrists in regard to chronically ill patients, concluding with an important consideration of semantic disciplines in planning a medical and psychological program for the chronically ill.

It was the consensus of the conferees that further discussions on medical and psychological teamwork in the care of the chronically ill should be held. These future sessions might be extended to include representation from nursing, social service, physical therapy, occupational therapy, pastoral guidance, and hospital administration. Such future conferences might be able to develop specific programs for training, research and actual practice in medico-psychological teamwork on behalf of the chronically ill.

If the high idealism of this conference can be extended, so that it may influence physicians and clinical psychologists toward a

more realistic understanding of the problems of chronic illness, it will have well achieved its purpose. Thanks to the radiation of Frank Fremont-Smith's "Free Floating Security" a real start seems to have been made in getting the ingredients of teamwork on the table so that internists and clinical psychologists and psychiatrists can see how to use them for the benefit of the chronically ill. The conference seems to have succeeded in evoking that good will and mutual understanding and respect between representatives of these disciplines that are prime essentials in working together successfully.

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